Dr Clarke
from the author
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ACCOUNT

OF A

SERIES OF EXPERIMENTS,

SHEWING THE EFFECTS OF

COMPRESSION

IN

MODIFYING THE ACTION OF HEAT.

BY

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ACCOUNT

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I.

Ancient Revolutions of the Mineral Kingdom.—Vain attempts to explain them.—Dependence of Geology on Chemiſtry.—Importance of the Carbonate of Lime.—Dr Black's *diſcovery of Carbonic Acid, ſubverted the former theories depending on Fire, but gave birth to that of Dr* Hutton.—*Progreſs of the Author's Ideas with regard to that Theory.—Experiments with Heat and Compreſſion, ſuggeſted to Dr* Hutton *in* 1790.—*Undertaken by the Author in* 1798.—*Speculations on which his hopes of ſucceſs were founded.*

WHOEVER has attended to the ſtructure of Rocks and Mountains, muſt be convinced, that our Globe has not always exiſted in its preſent ſtate; but that every part of its maſs, ſo far at leaſt as our obſervations reach, has been agitated and ſubverted by the moſt violent revolutions.

Facts leading to ſuch ſtriking concluſions, however imperfectly obſerved, could not fail to awaken curioſity, and give riſe to a deſire of tracing the hiſtory, and of inveſtigating the cauſes, of ſuch ſtupendous events; and various attempts were made in this way, but with little ſucceſs; for while diſcoveries of of the utmoſt importance and accuracy were made in Aſtronomy and Natural Philoſophy, the ſyſtems produced by the Geologiſts were ſo fanciful and puerile, as ſcarcely to deſerve a ſerious refutation.

One principal cauſe of this failure, ſeems to have lain in the very imperfect ſtate of Chemiſtry, which has only of late years begun to deſerve the name of a ſcience. While Chemiſtry was in its infancy, it was impoſſible that Geology ſhould make any progreſs; ſince ſeveral of the moſt important circumſtances to be accounted for by this latter ſcience, are admitted on all hands to depend upon principles of the former. The conſolidation of looſe ſand into ſtrata of ſolid rock; the cryſtalline arrangement of ſubſtances accompanying thoſe ſtrata, and blended with them in various modes, are circumſtances of a chemical nature, which all thoſe who have attempted to frame theories of the earth have endeavoured by chemical reaſonings to reconcile to their hypotheſes.

Fire and *Water*, the only agents in nature by which ſtony ſubſtances are produced, under our obſervation, were employed by contending ſects of geologiſts, to explain all the phenomena of the mineral kingdom.

But the known properties of Water, are quite repugnant to the belief of its univerſal influence, ſince a very great proportion of the ſubſtances under conſideration are inſoluble, or nearly ſo, in that fluid; and ſince, if they were all extremely ſoluble, the quantity of water which is known to exiſt, or that could poſſibly exiſt in our planet, would be far too ſmall to accompliſh the office aſſigned to it in the Neptunian theory *. On the other hand, the known properties of Fire are no leſs inadequate to the purpoſe; for, various ſubſtances which frequently occur in the mineral kingdom, ſeem, by their preſence, to preclude

* *Illuſtrations of the Huttonian Theory*, by Mr Profeſſor Playfair, § 430.

clude its ſuppoſed agency; ſince experiment ſhews, that, in our fires, they are totally changed or deſtroyed.

Under ſuch circumſtances, the advocates of either element were enabled, very ſucceſsfully, to refute the opinions of their adverſaries, though they could but feebly defend their own: and, owing perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both ſyſtems maintained a certain degree of credit; and writers on geology indulged themſelves, with a ſort of impunity, in a ſtyle of unphiloſophical reaſoning, which would not have been tolerated in other ſciences.

Of all mineral ſubſtances, the *Carbonate of Lime* is unqueſtionably the moſt important in a general view. As limeſtone or marble, it conſtitutes a very conſiderable part of the ſolid maſs of many countries; and, in the form of veins and nodules of ſpar, pervades every ſpecies of ſtone. Its hiſtory is thus interwoven in ſuch a manner with that of the mineral kingdom at large, that the fate of any geological theory muſt very much depend upon its ſucceſsful application to the various conditions of this ſubſtance. But, till Dr Black, by his diſcovery of Carbonic Acid, explained the chemical nature of the carbonate, no rational theory could be formed, of the chemical revolutions which it has undoubtedly undergone.

This diſcovery was, in the firſt inſtance, hoſtile to the ſuppoſed action of fire; for the decompoſition of limeſtone by fire in every common kiln being thus proved, it ſeemed abſurd to aſcribe to that ſame agent the formation of limeſtone, or of any maſs containing it.

The contemplation of this difficulty led Dr Hutton to view the action of fire in a manner peculiar to himſelf, and thus to form a geological theory, by which, in my opinion, he has furniſhed the world with the true ſolution of one of the moſt intereſting problems that has ever engaged the attention of men of ſcience.

He ſuppoſed,

I. That Heat has acted, at ſome remote period, on all rocks.

II. That during the action of heat, all theſe rocks (even ſuch as now appear at the ſurface) lay covered by a ſuperincumbent maſs, of great weight and ſtrength.

III. That in conſequence of the combined action of Heat and Preſſure, effects were produced different from thoſe of heat on common occaſions; in particular, that the carbonate of lime was reduced to a ſtate of fuſion, more or leſs complete, without any calcination.

The eſſential and characteriſtic principle of his theory is thus compriſed in the word *Compreſſion;* and by one bold hypotheſis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for thoſe circumſtances in which minerals are found to differ from the uſual products of our furnaces.

This ſyſtem, however, involves ſo many ſuppoſitions, apparently in contradiction to common experience, which meet us on the very threſhold, that moſt men have hitherto been deterred from the inveſtigation of its principles, and only a few individuals have juſtly appreciated its merits. It was long before I belonged to the latter claſs; for I muſt own, that, on reading Dr Hutton's firſt geological publication, I was induced to reject his ſyſtem entirely, and ſhould probably have continued ſtill to do ſo, with the great majority of the world, but for my habits of intimacy with the author; the vivacity and perſpicuity of whoſe converſation, formed a ſtriking contraſt to the obſcurity

ſcurity of his writings. I was induced by that charm, and by the numerous original facts which his ſyſtem had led him to obſerve, to liſten to his arguments, in favour of opinions which I then looked upon as viſionary. I thus derived from his converſation, the ſame advantage which the world has lately done from the publication of Mr PLAYFAIR's *Illuſtrations;* and, experienced the ſame influence which is now exerted by that work, on the minds of our moſt eminent men of ſcience.

AFTER three years of almoſt daily warfare with Dr HUTTON, on the ſubject of his theory, I began to view his fundamental principles with leſs and leſs repugnance. There is a period, I believe, in all ſcientific inveſtigations, when the conjectures of genius ceaſe to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, againſt its improbability as an hypotheſis: The partial view which we then obtain of truth, is perhaps the moſt attractive of any, and moſt powerfully ſtimulates the exertions of an active mind. The miſt which obſcured ſome objects diſſipates by degreee, and allows them to appear in their true colours; at the ſame time, a diſtant proſpect opens to our view, of ſcenes unſuſpected before.

ENTERING now ſeriouſly into the train of reaſoning followed by Dr HUTTON, I conceived that the chemical effects aſcribed by him to compreſſion, ought, in the firſt place, to be inveſtigated; for, unleſs ſome good reaſon were given us for believing that heat would be modified by preſſure, in the manner alleged, it would avail us little to know that they had acted together. He reſted his belief of this influence on analogy; and on the ſatisfactory ſolution of all the phenomena, furniſhed by this ſuppoſition. It occurred to me, however, that this principle was ſuſceptible of being eſtabliſhed in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this propoſal, on account of the immenſity of the natural agents, whoſe operations he ſuppoſed to lie far beyond the reach of our imitation; and he ſeemed to imagine, that any ſuch attempt muſt undoubtedly fail, and thus throw diſcredit on opinions already ſufficiently eſtabliſhed, as he conceived, on other principles. I was far, however, from being convinced by theſe arguments; for, without being able to prove that any artificial compreſſion to which we could expoſe the carbonate, would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might poſſibly perform all that was hypothetically aſſumed in the Huttonian Theory. On the other hand, I conſidered myſelf as bound, in practice, to pay deference to his opinion, in a field which he had already ſo nobly occupied, and abſtained, during the remainder of his life, from the proſecution of ſome experiments with compreſſion, which I had begun in 1790.

In 1798, I reſumed the ſubject with eagerneſs, being ſtill of opinion, that the chemical law which forms the baſis of the Huttonian Theory, ought, in the firſt place, to be inveſtigated experimentally; all my ſubſequent reflections and obſervations having tended to confirm my idea of the importance of this purſuit, without in any degree rendering me more apprehenſive as to the reſult.

In the arrangement of the following paper, I ſhall firſt confine myſelf to the inveſtigation of the chemical effects of Heat and Compreſſion, reſerving to the concluding part, the application of my reſults to Geology. I ſhall, then, appeal to the volcanoes, and ſhall endeavour to vindicate the laws of action aſſumed in the Huttonian Theory, by ſhewing, that lavas, previous to their eruptions, are ſubject to ſimilar laws; and that the volcanoes, by their ſubterranean and ſubmarine exertions,

tions, muſt produce, in our times, reſults ſimilar to thoſe aſcribed, in that Theory, to the former action of fire.

In comparing the Huttonian operations with thoſe of the volcanoes, I ſhall avail myſelf of ſome facts, brought to light in the courſe of the following inveſtigations, by which a preciſe limit is aſſigned to the intenſity of the heat, and to the force of compreſſion, required to fulfil the conditions of Dr Hutton's hypotheſis: For, according to him, the power of thoſe agents was very great, but quite indefinite; it was therefore impoſſible to compare their ſuppoſed effects in any preciſe manner with the phenomena of nature.

My attention was almoſt excluſively confined to the Carbonate of Lime, about which I reaſoned as follows: The carbonic acid, when uncombined with any other ſubſtance, exiſts naturally in a gaſeous form, at the common temperature of our atmoſphere; but when in union with lime, its volatility is repreſſed, in that ſame temperature, by the chemical force of the earthy ſubſtance, which retains it in a ſolid form. When the temperature is raiſed to a full red-heat, the acid acquires a volatility by which that force is overcome, it eſcapes from the lime, and aſſumes its gaſeous form. It is evident, that were the attractive force of the lime increaſed, or the volatility of the acid diminiſhed by any means, the compound would be enabled to bear a higher heat without decompoſition, than it can in the preſent ſtate of things. Now, preſſure muſt produce an effect of this kind; for when a mechanical force oppoſes the expanſion of the acid, its volatility muſt, to a certain degree, be diminiſhed. Under preſſure, then, the carbonate may be expected to remain unchanged in a heat, by which, in the open air, it would have been calcined. But experiment alone can teach us, what compreſſing force is requiſite to enable it to reſiſt any given elevation of temperature; and what is to be the reſult of ſuch an operation. Some of the compounds of lime with acids

are

are fusible, others refractory; the carbonate, when constrained by pressure to endure a proper heat, may be as fusible as the muriate.

One circumstance, derived from the Huttonian Theory, induced me to hope, that the carbonate was easily fusible, and indicated a precise point, under which that fusion ought to be expected. Nothing is more common than to meet with nodules of calcareous spar inclosed in whinstone; and we suppose, according to the Huttonian Theory, that the whin and the spar had been liquid together; the two fluids keeping separate, like oil and water. It is natural, at the junction of these two, to look for indications of their relative fusibilities; and we find, accordingly, that the termination of the spar is generally globular and smooth; which seems to prove, that, when the whin became solid, the spar was still in a liquid state; for had the spar congealed first, the tendency which it shews, on all occasions of freedom, to shoot out into prominent crystals, would have made it dart into the liquid whin, according to the peculiar forms of its crystallization; as has happened with the various substances contained in whin, much more refractory than itself, namely, augite, felspar, &c.; all of which having congealed in the liquid whin, have assumed their peculiar forms with perfect regularity. From this I concluded, that when the whin congealed, which must have happened about 28° or 30° of Wedgwood, the spar was still liquid. I therefore expected, if I could compel the carbonate to bear a heat of 28° without decomposition, that it would enter into fusion. The sequel will shew, that this conjecture was not without foundation.

I shall now enter upon the description of those experiments, the result of which I had the honour to lay before this Society on the 30th of August last (1804); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various

ous particulars, to fteer between the oppofite faults of prolixity and barrennefs. My object fhall be to defcribe, as fhortly as poffible, all the methods followed, fo as to enable any chemift to repeat the experiments; and to dwell particularly on fuch circumftances only, as feem to lead to conclufions of importance.

The refult being already known, I confider the account I am about to give of the execution of thefe experiments, as addreffed to thofe who take a particular intereft in the progrefs of chemical operations: in the eyes of fuch gentlemen, I truft, that none of the details into which I muft enter, will appear fuperfluous.

II.

Principle of execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the muzzle.—Method with the Fufible Metal.—Remarkable effects of its expanfion.—Neceffity of introducing Air.—Refults obtained.

When I firft undertook to make experiments with heat acting under compreffion, I employed myfelf in contriving various devices of fcrews, of bolts, and of lids, fo adjufted, I hoped, as to confine all elaftic fubftances; and perhaps fome of them might have anfwered. But I laid afide all fuch devices, in favour of one which occurred to me in January 1798; which, by its fimplicity, was of eafy application in all cafes, and accomplifhed all that could be done by any device, fince it fecured perfect ftrength and tightnefs to the utmoft that the veffels employed could bear, whether formed of metallic or earthy fubftance. The device depends upon the

the following general view: If we take a hollow tube or barrel (AD, fig. 1.) clofed at one end, and open at the other, of one foot or more in length; it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if neceffary, expofed to extreme cold. If, then, the fubftance which we mean to fubject to the combined action of heat and preffure, be introduced into the breech or clofed end of the barrel (CD), and if the middle part be filled with fome refractory fubftance, leaving a fmall empty fpace at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the fubject of experiment, is kept cool, and thus clofe the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of fealing-wax. Things being then reverfed, and the breech put into the furnace, a heat of any required intenfity may be applied to the fubject of experiment, now in a ftate of conftraint.

My firft application of this fcheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very ftrongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previoufly rammed into a cartridge of paper or pafteboard, in order to protect it from the iron, by which, in fome former trials, the fubject of experiment had been contaminated throughout during the action of heat. I then rammed the reft of the barrel full of pounded clay, previoufly baked in a ftrong heat, and I had the muzzle clofed like the breech, by a plug of iron welded upon it in a common forge; the reft of the barrel being kept cold during this operation, by means of wet cloths. The breech of the barrel was then introduced horizontally into a common muffle, heated to about 25° of Wedgwood. To the muzzle a rope was fixed, in fuch a manner, that the barrel could be withdrawn without

out danger from an explofion *. I likewife, about this time, clofed the muzzle of the barrel, by means of a plug, fixed by folder only ; which method had this peculiar advantage, that I could fhut and open the barrel, without having recourfe to a workman. In thefe trials, though many barrels yielded to the expanfive force, others refifted it, and afforded fome refults that were in the higheft degree encouraging, and even fatisfactory, could they have been obtained with certainty on repetition of the procefs. In many of them, chalk, or common limeftone previoufly pulverifed, was agglutinated into a ftony mafs, which required a fmart blow of a hammer to break it, and felt under the knife like a common limeftone; at the fame time, the fubftance, when thrown into nitric acid, diffolved entirely with violent effervefcence.

In one of thefe experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been ufed to fill the barrel, was ftained black throughout, to the diftance of two-thirds of the length of the barrel from its breech. This circumftance is of importance, by fhewing, that though all is tight at the muzzle, a protrufion may take place along the barrel, greatly to the detriment of complete

* On one occafion, the importance of this precaution was ftrongly felt. Having inadvertently introduced a confiderable quantity of moifture into a welded barrel, an explofion took place, before the heat had rifen to rednefs, by which, part of the barrel was fpread out to a flat plate, and the furnace was blown to pieces. Dr Kennedy, who happened to be prefent on this occafion, obferved, that notwithftanding this accident, the time might come when we fhould employ water in thefe experiments to affift the force of compreffion. I have fince made great ufe of this valuable fuggeftion: but he fcarcely lived, alas! to fee its application; for my firft fuccefs in this way, took place during his laft illnefs.—I have been expofed to no rifk in any other experiment with iron barrels; matters being fo arranged, that the ftrain againft them has only commenced in a red heat, in which the metal has been fo far foftened, as to yield by laceration like a piece of leather.

plete compreffion: and, at the fame time, it illuftrates what has happened occafionally in nature, where the bituminous matter feems to have been driven by fuperior local heat, from one part of a coaly bed, though retained in others, under the fame compreffion. The bitumen fo driven off being found, in other cafes, to pervade and tinge beds of flate and of fandftone.

I WAS employed in this purfuit in fpring 1800, when an event of importance interrupted my experiments for about a year. But I refumed them in March 1801, with many new plans of execution, and with confiderable addition to my apparatus.

IN the courfe of my firft trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemifts, that a certain compofition of different metals *, produces a fubftance fo fufible, as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and difpatch, might be gained in thefe experiments, by fubftituting this metal for the baked clay above mentioned: That after introducing the carbonate into the breech of the barrel, the fufible metal, in a liquid ftate, might be poured in, fo as to fill the barrel to its brim: That when the metal had cooled and become folid, the breech might, as before, be introduced into a muffle, and expofed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fufible metal being melted, but what lay at the breech, the reft, continuing in a folid ftate, would effectually confine the carbonic acid: That after the action of ftrong heat had ceafed, and after all had been allowed to cool completely, the fufible metal might be removed entirely from the barrel, by means of a heat little above that of boiling water, and far too low to occafion any decompofition of the

* Eight parts of bifmuth, five of lead, and three of tin.

the carbonate by calcination, though acting upon it in freedom; and then, that the ſubject of experiment might, as before, be taken out of the barrel.

This ſcheme, with various modifications and additions, which practice has ſuggeſted, forms the baſis of moſt of the following methods.

In the firſt trial, a ſtriking phenomenon occurred, which gave riſe to the moſt important of theſe modifications. Having filled a gun-barrel with the fuſible metal, without any carbonate; and having placed the breech in a muffle, I was ſurpriſed to ſee, as the heat approached to redneſs, the liquid metal exuding through the iron in innumerable minute drops, diſperſed all round the barrel. As the heat advanced, this exudation increaſed, till at laſt the metal flowed out in continued ſtreams, and the barrel was quite deſtroyed. On ſeveral occaſions of the ſame kind, the fuſible metal, being forced through ſome very minute aperture in the barrel, ſpouted from it to the diſtance of ſeveral yards, depoſiting upon any ſubſtance oppoſed to the ſtream, a beautiful aſſemblage of fine wire, exactly in the form of wool. I immediately underſtood, that the phenomenon was produced by the ſuperior expanſion of the liquid over the ſolid metal, in conſequence of which, the fuſible metal was driven through the iron as water was driven through ſilver * by mechanical percuſſion in the Florentine experiment. It occurred to me, that this might be prevented by confining along with the fuſible metal a ſmall quantity of air, which, by yielding a little to the expanſion of the liquid, would ſave the barrel. This remedy

* *Eſſays of Natural Experiments made in the Academie del Cimento*, tranſlated by Waller, London, 1684, page 117. The ſame in Musschenbroek's Latin tranſlation, Lugd. Bat. 1731, p. 63.

medy was found to anſwer completely, and was applied, in all the experiments made at this time *.

I NOW propoſed, in order to keep the carbonate clean, to incloſe it in a ſmall veſſel; and to obviate the difficulty of removing the reſult at the concluſion of the experiment, I further propoſed to connect that veſſel with an iron ramrod, longer than the barrel, by which it could be introduced or withdrawn at pleaſure.

A SMALL tube of glaſs †, or of Raumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length, (fig. 2. A) was half filled with pounded carbonate of lime, rammed as hard as poſſible; the other half of the tube being

* I found it a matter of much difficulty to aſcertain the proper quantity of air which ought to be thus incloſed. When the quantity was too great, the reſult was injured by diminution of elaſticity, as I ſhall have occaſion fully to ſhew hereafter. When too ſmall, or when, by any accident, the whole of this included air was allowed to eſcape, the barrel was deſtroyed.

I hoped to aſcertain the bulk of air neceſſary to give liberty to the expanſion of the liquid metal, by meaſuring the actual quantity expelled by known heats from an open barrel filled with it. But I was ſurpriſed to find, that the quantity thus diſcharged, exceeded in bulk that of the air which, in the ſame heats, I had confined along with the carbonate and fuſible metal in many ſucceſsful experiments. As the expanſion of the liquid does not ſeem capable of ſenſible diminution by an oppoſing force, this fact can only be accounted for by a diſtention of the barrel. In theſe experiments, then, the expanſive force of the carbonic acid, of the included air, and of the fuſible metal, acted in combination againſt the barrel, and were yielded to in part by the diſtention of the barrel, and by the condenſation of the included air. My object was to increaſe the force of this mutual action, by diminiſhing the quantity of air, and by other devices to be mentioned hereafter. Where ſo many forces were concerned, the laws of whoſe variations were unknown, much preciſion could not be expected, nor is it wonderful, that in attempting to carry the compreſſing force to the utmoſt, I ſhould have deſtroyed barrels innumerable.

† I have ſince conſtantly uſed tubes of common porcelain, finding glaſs much too fuſible for this purpoſe.

being filled with pounded filex, or with whatever occurred as moft likely to prevent the intrufion of the fufible metal in its liquid and penetrating ftate. This tube fo filled, was placed in a frame or cradle of iron (*d f k h*, figs. 3, 4, 5, and 6,) fixed to the end (*m*) of a ram-rod (*m n*). The cradle was from fix to three inches in length, and as much in diameter as a gun-barrel would admit with eafe. It was compofed of two circular plates of iron, (*d e f g* and *h i k l*, feen edgewife in the figures,) placed at right-angles to the ram-rod, one of thefe plates (*d e f h*) being fixed to it by the centre (*m*). Thefe plates were connected together by four ribs or flattened wires of iron (*d h*, *e i*, *f k*, and *g l*,) which formed the cradle into which the tube (A), containing the carbonate, was introduced by thrufting the adjacent ribs afunder. Along with the tube juft mentioned, was introduced another tube (B), of iron or porcelain, filled only with air. Likewife, in the cradle, a pyrometer * piece (C) was placed in contact with (A) the tube containing the carbonate. Thefe articles generally occupied the

* THE pyrometer-pieces ufed in thefe experiments were made under my own eye. Neceffity compelled me to undertake this laborious and difficult work, in which I have already fo far fucceeded as to obtain a fet of pieces, which, though far from complete, anfwer my purpofe tolerably well. I had lately an opportunity of comparing my fet with that of Mr WEDGWOOD, at various temperatures, in furnaces of great fize and fteadinefs. The refult has proved, that my pieces agree as well with each other as his, though with my fet each temperature is indicated by a different degree of the fcale. I have thus been enabled to conftruct a table, by which my obfervations have been corrected, fo that the temperatures mentioned in this paper are fuch as would have been indicated by Mr WEDGWOOD's pieces. By Mr WEDGWOOD's pieces, I mean thofe of the only fet which has been fold to the public, and by which the melting heat of pure filver is indicated at the 22d degree. I am well aware, that the late Mr WEDGWOOD, in his Table of Fufibilities, has ftated that fufion as taking place at the 28th degree; but I am convinced that his obfervations muft have been made with fome fet different from that which was afterwards fold,

the whole cradle ; when any ſpace remained, it was filled up by a piece of chalk dreſſed for the purpoſe. (Fig. 4. repreſents the cradle filled, as juſt deſcribed).

Things being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fuſible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, ſo that the carbonate was placed very near the breech, (as repreſented in fig. 5, the fuſible metal ſtanding at *o*). The air-tube (B) being placed ſo as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though ſome of it might be driven off by the heat, ſo as to eſcape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held faſt. The barrel being then filled to the brim with fuſible metal, the apparatus was ready for the application of heat to the breech, (as ſhewn in fig. 6.)

In the experiments made at this time, I uſed a ſquare brick furnace (figs. 7 and 8), having a muffle (*r s*) traverſing it horizontally and open at both ends. This muffle being ſupported in the middle by a very ſlender prop, was expoſed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hotteſt part, and the end next the muzzle projecting beyond the furnace, and ſurrounded with cloths which were drenched with water from time to time. (This arrangement is ſhewn in fig. 7). In this ſituation, the fuſible metal ſurrounding the cradle being melted, the air contained in the air-tube would of courſe ſeek the higheſt poſition, and its firſt place in the air-tube would be occupied by fuſible metal. (In fig. 6., the new poſition of the air is ſhewn at *p q*).

At

At the conclufion of the experiment, the metal was generally removed by placing the barrel in the tranfverfe muffle, with its muzzle pointing a little downwards, and fo that the heat was applied firft to the muzzle, and then to the reft of the barrel in fucceffion. (This operation is fhewn in fig. 8). In fome of the firft of thefe experiments, I loofened the cradle, by plunging the barrel into heated brine, or a ftrong folution of muriate of lime; which laft bears a temperature of 250° of Fahrenheit before it boils. For this purpofe, I ufed a pan three inches in diameter, and three feet deep, having a flat bafon at top to receive the liquid when it boiled over. The method anfwered, but was troublefome, and I laid it afide. I have had occafion, lately, however, to refume it in fome experiments in which it was of confequence to open the barrel with the leaft poffible heat *.

By thefe methods I made a great number of experiments, with refults that were highly interefting in that ftage of the bufinefs, though their importance is fo much diminifhed by the fubfequent progrefs of the inveftigation, that I think it proper to mention but very few of them.

On the 31ft of March 1801, I rammed forty grains of pounded chalk into a tube of green bottle-glafs, and placed it in the cradle as above defcribed. A pyrometer in the muffle along with the barrel indicated 33°. The barrel was expofed to heat during feventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one folid mafs, which had vifibly fhrunk in bulk, the fpace thus left within the tube being accurately

* In many of the following experiments, lead was ufed in place of the fufible metal, and often with fuccefs; but I loft many good refults in this way: for the heat required to liquefy the lead, approaches fo near to rednefs, that it is difficult to difengage the cradle without applying a temperature by which the carbonate is injured. I have found it anfwer well, to furround the cradle and a few inches of the rod, with fufible metal, and to fill the reft of the barrel with lead.

accurately filled with metal, which plated the carbonate all over without penetrating it in the leaſt, ſo that the metal was eaſily removed. The weight was reduced from forty to thirty-ſix grains. The ſubſtance was very hard, and reſiſted the knife better than any reſult of the kind previouſly obtained; its fracture was cryſtalline, bearing a reſemblance to white ſaline marble; and its thin edges had a decided ſemitranſparency, a circumſtance firſt obſerved in this reſult.

On the 3d of March of the ſame year, I made a ſimilar experiment, in which a pyrometer-piece was placed within the barrel, and another in the muffle; they agreed in indicating 23°. The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have loſt 3½ grains. A thin rim, leſs than the 20th of an inch in thickneſs, of whitiſh matter, appeared on the outſide of the maſs. In other reſpects, the carbonate was in a very perfect ſtate; it was of a yellowiſh colour, and had a decided ſemitranſparency and ſaline fracture. But what renders this reſult of the greateſt value, is, that on breaking the maſs, a ſpace of more than the tenth of an inch ſquare, was found to be completely cryſtallized, having acquired the rhomboidal fracture of calcareous ſpar. It was white and opaque, and preſented to the view three ſets of parallel plates which are ſeen under three different angles. This ſubſtance, owing to partial calcination and ſubſequent abſorption of moiſture, had loſt all appearance of its remarkable properties in ſome weeks after its production; but this appearance has ſince been reſtored, by a freſh fracture, and the ſpecimen is now well preſerved by being hermetically incloſed.

III.

III.

Experiments made in Tubes of Porcelain.—Tubes of Wedgwood's Ware.—Methods uſed to confine the Carbonic Acid, and to cloſe the Pores of the Porcelain in a Horizontal Apparatus.—Tubes made with a view to theſe Experiments.—The Vertical Apparatus adopted.—View of Reſults obtained, both in Iron and Porcelain.—The Formation of Lime-ſtone and Marble.—Inquiry into the Cauſe of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking.—Experiments with Porcelain Tubes proved to be limited.

WHILE I was carrying on the above-mentioned experiments, I was occaſionally occupied with another ſet, in tubes of porcelain. So much, indeed, was I prepoſſeſſed in favour of this laſt mode, that I laid gun-barrels aſide, and adhered to it during more than a year. The methods followed with this ſubſtance, differ widely from thoſe already deſcribed, though founded on the ſame general principles.

I PROCURED from Mr WEDGWOOD's manufactory at Etruria, in Staffordſhire, a ſet of tubes for this purpoſe, formed of the ſame ſubſtance with the white mortars, in common uſe, made there. Theſe tubes were fourteen inches long, with a bore of half an inch diameter, and thickneſs of 0.2; being cloſed at one end (figs. 9, 10, 11, 12, 13.)

I PROPOSED to ram the carbonate of lime into the breech (Fig. 9. A); then filling the tube to within a ſmall diſtance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the ſhops (borat of ſoda) previouſly reduced to glaſs, and then pounded; to apply heat to the muzzle alone, ſo as to convert that borax into ſolid glaſs; then, reverſing the operation, to keep the muzzle cold, and apply the requiſite heat to the carbonate lodged in the breech.

I THUS expected to confine the carbonic acid; but the attempt was attended with confiderable difficulty, and has led to the employment of various devices, which I fhall now fhortly enumerate, as they occurred in the courfe of practice. The fimple application of the principle was found infufficient, from two caufes: Firft, The carbonic acid being driven from the breech of the tube, towards the muzzle, among the pores of the pounded filex, efcaped from the compreffing force, by lodging itfelf in cavities which were comparatively cold: Secondly, The glafs of borax, on cooling, was always found to crack very much, fo that its tightnefs could not be depended on.

To obviate both thefe inconveniences at once, it occurred to me, in addition to the firft arrangement, to place fome borax (fig. 10. C) fo near the breech of the tube, as to undergo heat along with the carbonate (A); but interpofing between this borax and the carbonate, a ftratum of filex (B), in order to prevent contamination. I trufted that the borax in a liquid or vifcid ftate, being thruft outwards by the expanfion of the carbonic acid, would prefs againft the filex beyond it (D), and totally prevent the elaftic fubftances from efcaping out of the tube, or even from wandering into its cold parts.

IN fome refpects, this plan anfwered to expectation. The glafs of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued vifcid mafs in the loweft red-heat; and as the ftrefs in thefe experiments, begins only with rednefs, the borax being heated at the fame time with the carbonate, becomes united and impervious, as foon as its action is neceffary. Many good refults were accordingly obtained in this way. But I found, in practice, that as the heat rofe, the borax began to enter into too thin fufion, and was often loft among the pores of the filex, the fpace in which it had lain being found empty on breaking the tube. It was therefore

therefore found neceſſary to oppoſe ſomething more ſubſtantial and compact, to the thin and penetrating quality of pure borax.

In ſearching for ſome ſuch ſubſtance, a curious property of bottle-glaſs occurred accidentally. Some of this glaſs, in powder, having been introduced into a muffle at the temperature of about 20° of Wedgwood; the powder, in the ſpace of about a minute, entered into a ſtate of viſcid agglutination, like that of honey, and in about a minute more, (the heat always continuing unchanged,) conſolidated into a firm and compact maſs of *Reaumur's porcelain* *. It now appeared, that by placing this ſubſtance immediately behind the borax, the penetrating quality of this laſt might be effectually reſtrained; for, Reaumur's porcelain has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of conſolidation, the pounded bottle-glaſs ſhrunk, ſo as to leave an opening between its maſs and the tube, through which the borax, and, along with it, the carbonic acid, was found to eſcape. But the object in view was obtained by means of a mixture of pounded bottle-glaſs, and pounded flint, in equal parts. This compound ſtill agglutinates, not indeed into a maſs ſo hard as Reaumur's porcelain, but ſufficiently ſo for the purpoſe; and this being done without any ſenſible contraction, an effectual barrier was oppoſed to the borax; (this arrangement is ſhewn in fig. 11.); and thus the method of cloſing the tubes was rendered ſo complete, as ſeldom to fail in practice †. A ſtill further refinement upon this me-

* In the ſame temperature, a maſs of the glaſs of equal bulk would undergo the ſame change; but it would occupy an hour.

† A ſubſtance equally efficacious in reſtraining the penetrating quality of borax, was diſcovered by another accident. It conſiſts of a mixture of borax and common ſand, by which a ſubſtance is formed, which, in heat, aſſumes the ſtate of a very tough paſte, and becomes hard and compact on cooling.

thod was found to be of advantage. A ſecond ſeries of powders, like that already deſcribed, was introduced towards the muzzle, (as ſhewn in fig. 12.). During the firſt period of the experiment, this laſt-mentioned ſeries was expoſed to heat, with all the outward half of the tube (*a b*); by this means, a ſolid maſs was produced, which remained cold and firm during the ſubſequent action of heat upon the carbonate.

I soon found, that notwithſtanding all the above-mentioned precautions, the carbonic acid made its eſcape, and that it pervaded the ſubſtance of the Wedgwood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating ſtate of fuſion, applied to the inſide of the tube; and that the pores of the porcelain might thus be cloſed, as thoſe of leather are cloſed by oil, in an air-pump. In this view, I rammed the carbonate into a ſmall tube, and ſurrounded it with pounded glaſs of borax, which, as ſoon as the heat was applied, ſpread on the inſide of the large tube, and effectually cloſed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffles, (the arrangement juſt deſcribed being repreſented in fig. 13.)

I was thus enabled to carry on experiments with this porcelain, to the utmoſt that its ſtrength would bear. But I was not ſatisfied with the force ſo exerted; and, hoping to obtain tubes of a ſuperior quality, I ſpent much time in experiments with various porcelain compoſitions. In this, I ſo far ſucceeded, as to produce tubes by which the carbonic acid was in a great meaſure retained without any internal glaze. The beſt material I found for this purpoſe, was the pure porcelain-clay of Cornwall, or a compoſition in the proportion of two of this clay to one of what the potters call *Corniſh-ſtone*, which I believe to be a granite in a ſtate of decompoſition. Theſe tubes were ſeven or eight inches long, with a bore

tapering

tapering from 1 inch to 0.6. Their thicknefs was about 0.3 at the breech, and tapered towards the muzzle to the thinnefs of a wafer.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally, as before. By obferving the thin ftate of borax whilft in fufion, I was convinced, that it ought to be treated as a complete liquid, which being fupported in the courfe of the experiment from below, would fecure perfect tightnefs, and obviate the failure which often happened in the horizontal pofition, from the falling of the borax to the lower fide.

In this view, (fig. 16.), I filled the breech in the manner defcribed above, and introduced into the muzzle fome borax (C) fupported at the middle of the tube by a quantity of filex mixed with bottle-glafs (B). I placed the tube, fo prepared, with its breech plunged into a crucible filled with fand (E), and its muzzle pointing upwards. It was now my object to apply heat to the muzzle-half, whilft the other remained cold. In that view, I conftructed a furnace (fig. 14. and 15.), having a muffle placed vertically (*c d*), furrounded on all fides with fire (*e e*), and open both above (at *c*), and below (at *d*). The crucible juft mentioned, with its tube, being then placed on a fupport directly below the vertical muffle, (as reprefented in fig. 14. at F), it was raifed, fo that the half of the tube next the muzzle was introduced into the fire. In confequence of this, the borax was feen from above to melt, and run down in the tube, the air contained in the powder efcaping in the form of bubbles, till at laft the borax ftood with a clear and fteady furface like that of water. Some of this falt being thrown in from above, by means of a tube of glafs, the liquid furface was raifed nearly to the muzzle, and, after all had been allowed to become cold, the pofition of the tube was reverfed; the muzzle being now plun-

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ged into the ſand, (as in fig. 17.), and the breech introduced into the muffle. In ſeveral experiments, I found it anſwer well, to occupy great part of the ſpace next the muzzle, with a rod of ſand and clay previouſly baked, (fig. 19. K K), which was either introduced at firſt, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid ſtate. In many caſes I aſſiſted the compactneſs of the tube by means of an internal glaze of borax; the carbonate being placed in a ſmall tube, (as ſhewn in fig. 18.)

These devices anſwered the end propoſed. Three-fourths of the tube next the muzzle was found completely filled with a maſs, having a concave termination at both ends, (*f* and *g* figs. 17. 18. 19.), ſhewing that it had ſtood as a liquid in the two oppoſite poſitions in which heat had been applied to it. So great a degree of tightneſs indeed was obtained in this way, that I found myſelf ſubjected to an unforeſeen ſource of failure. A number of the tubes failed, not by exploſion, but by the formation of a minute longitudinal fiſſure at the breech, through which the borax and carbonic acid eſcaped. I ſaw that this aroſe from the expanſion of the borax when in a liquid ſtate, as happened with the fuſible metal in the experiments with iron-barrels; for, the crevice here formed, indicated the exertion of ſome force acting very powerfully, and to a very ſmall diſtance. Accordingly, this ſource of failure was remedied by the introduction of a very ſmall air-tube. This, however, was uſed only in a few experiments.

In the courſe of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above deſcribed, amounting, together with thoſe made in gun-barrels, to one hundred and fifty-ſix. In an operation ſo new, and in which the apparatus was ſtrained to the utmoſt of its power, conſtant ſucceſs could not be expected, and in fact many experiments failed, wholly or partially. The reſults, however, upon the

the whole, were ſatisfactory, ſince they ſeemed to eſtabliſh ſome of the eſſential points of this inquiry.

These experiments prove, that, by mechanical conſtraint, the carbonate of lime can be made to undergo ſtrong heat, without calcination, and to retain almoſt the whole of its carbonic acid, which, in an open fire, at the ſame temperature, would have been entirely driven off: and that, in theſe circumſtances, heat produces ſome of the identical effects aſcribed to it in the Huttonian Theory.

By this joint action of heat and preſſure, the carbonate of lime which had been introduced in the ſtate of the fineſt powder, is agglutinated into a firm maſs, poſſeſſing a degree of hardneſs, compactneſs, and ſpecific gravity *, nearly approaching to theſe qualities in a ſound limeſtone; and ſome of the reſults, by their ſaline fracture, by their ſemitranſparency, and their ſuſceptibility of poliſh, deſerve the name of marble.

The ſame trials have been made with all calcareous ſubſtances; with chalk, common limeſtone, marble, ſpar, and the ſhells of fiſh. All have ſhewn the ſame general property, with ſome varieties as to temperature. Thus, I found, that, in the ſame circumſtances, chalk was more ſuſceptible of agglutination than ſpar; the latter requiring a heat two degrees higher than the former, to bring it to the ſame pitch of agglutination.

The chalk uſed in my firſt experiments, always aſſumed the character of a yellow marble, owing probably to ſome ſlight contamination of iron. When a ſolid piece of chalk, whoſe bulk had been previouſly meaſured in the gage of Wedgwood's pyrometer was ſubmitted to heat under compreſſion, its contraction was remarkable, proving the approach of the particles during their conſolidation; on theſe occaſions, it was found to

* See Appendix.

to ſhrink three times more than the pyrometer-pieces in the ſame temperature. It loſt, too, almoſt entirely, its power of imbibing water, and acquired a great additional ſpecific gravity. On ſeveral occaſions, I obſerved, that maſſes of chalk, which, before the experiment, had ſhewn one uniform character of whiteneſs, aſſumed a ſtratified appearance, indicated by a ſeries of parallel layers of a brown colour. This circumſtance may hereafter throw light on the geological hiſtory of this extraordinary ſubſtance.

I HAVE ſaid, that, by mechanical conſtraint, almoſt the whole of the carbonic acid was retained. And, in truth, at this period, ſome loſs of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumſtance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

WHEN the loſs exceeded 10 or 15 *per cent* *. of the weight of the carbonate, the reſult was always of a friable texture, and without any ſtony character; when leſs than 2 or 3 *per cent.* it was conſidered as good, and poſſeſſed the properties of a natural carbonate. In the intermediate caſes, when the loſs amounted, for inſtance, to 6 or 8 *per cent.*, the reſult was ſometimes excellent at firſt, the ſubſtance bearing every appearance of ſoundneſs, and often poſſeſſing a high character of cryſtallization; but it was unable to reſiſt the action of the air; and, by attracting carbonic acid or moiſture, or both, crumbled to duſt more or leſs rapidly, according to circumſtances. This ſeems to prove, that the carbonate of lime, though not fully ſaturated with carbonic acid, may poſſeſs the properties of limeſtone; and perhaps a difference of this

* I have found, that, in open fire, the entire loſs ſuſtained by the carbonate varies in different kinds from 42 to 45.5 *per cent.*

this kind may exiſt among natural carbonates, give riſe to their different degrees of durability.

I HAVE obſerved, in many caſes, that the calcination has reached only to a certain depth into the maſs; the internal part remaining in a ſtate of complete carbonate, and, in general, of a very fine quality. The partial calcination ſeems thus to take place in two different modes. By one, a ſmall proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the reſt is left entire. Perhaps one reſult is the effect of a feeble calcining cauſe, acting during a long time, and the other of a ſtrong cauſe, acting for a ſhort time.

SOME of the reſults which ſeemed the moſt perfect when firſt produced, have been ſubject to decay, owing to partial calcination. It happened, in ſome degree, to the beautiful ſpecimen produced on the 3d of March 1801, though a freſh fracture has reſtored it.

A SPECIMEN, too, of marble, formed from pounded ſpar, on 15th May 1801, was ſo complete as to deceive the workman employed to poliſh it, who declared, that, were the ſubſtance a little whiter, the quarry from which it was taken would be of great value, if it lay within reach of a market. Yet, in a few weeks after its formation, it fell to duſt.

NUMBERLESS ſpecimens, however, have been obtained, which reſiſt the air, and retain their poliſh as well as any marble. Some of them continue in a perfect ſtate, though they have been kept without any precaution during four or five years. That ſet, in particular, remain perfectly entire, which were ſhewn laſt year in this Society, though ſome of them were made in 1799, ſome in 1801 and 1802, and though the firſt eleven were long ſoaked in water, in the trials made of their ſpecific gravity.

A curious circumftance occurred in one of thefe experiments, which may hereafter lead to important confequences. Some ruft of iron had accidentally found its way into the tube: 10 grains of carbonate were ufed, and a heat of 28° was applied. The tube had no flaw; but there was a certainty that the carbonic acid had efcaped through its pores. When broken, the place of the carbonate was found occupied, partly by a black flaggy matter, and partly by fphericles of various fizes, from that of a fmall pea downwards, of a white fubftance, which proved to be quicklime; the fphericles being interfperfed through the flag, as fpar and agates appear in whinftone. The flag had certainly been produced by a mixture of the iron with the fubftance of the tube; and the fpherical form of the quicklime feems to fhew, that the carbonate had been in fufion along with the flag, and that they had feparated on the efcape of the carbonic acid.

The fubject was carried thus far in 1803, when I fhould probably have publifhed my experiments, had I not been induced to profecute the inquiry by certain indications, and accidental refults, of a nature too irregular and uncertain to meet the public eye, but which convinced me, that it was poffible to eftablifh, by experiment, the truth of all that was hypothetically affumed in the Huttonian Theory.

The principal object was now to accomplifh the entire fufion of the carbonate, and to obtain fpar as the refult of that fufion, in imitation of what we conceive to have taken place in nature.

It was likewife important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itfelf, and on account of its confequences; the refult being vifibly improved by every approach towards complete faturation. I therefore became anxious to inveftigate the caufe of the partial calcinations which had always taken place, to

a

a greater or a lefs degree, in all thefe experiments. The queftion naturally fuggefts itfelf, What has become of the carbonic acid, feparated in thefe partial calcinations from the earthy bafis? Has it penetrated the veffel, and efcaped entirely, or has it been retained within it in a gafeous, but highly compreffed ftate? It occurred to me, that this queftion might be eafily refolved, by weighing the veffel before and after the action of heat upon the carbonate.

With iron, a conftant and inappreciable fource of irregularity exifted in the oxidation of the barrel. But with porcelain the thing was eafy; and I put it in practice in all my experiments with this material, which were made after the queftion had occurred to me. The tube was weighed as foon as its muzzle was clofed, and again, after the breech had been expofed to the fire; taking care, in both cafes, to allow all to cool. In every cafe, I found fome lofs of weight, proving, that even in the beft experiments, the tubes were penetrated to a certain degree. I next wifhed to try if any of the carbonic acid feparated, remained within the tube in a gafeous form; and in that view, I wrapt the tube, which had juft been weighed, in a fheet of paper, and placed it, fo furrounded, on the fcale of the balance. As foon as its weight was afcertained, I broke the tube by a fmart blow, and then replaced upon the fcale the paper containing all the fragments. In thofe experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already efcaped during the action of heat. But in the good refults, I always found that a lofs of weight was the confequence of breaking the tube.

These facts prove, that both caufes of calcination had operated in the porcelain tubes; that, in the cafes of fmall lofs, part of the carbonic acid had efcaped through the veffel, and that part had been retained within it. I had in view methods by which the laſt could be counteracted; but I ſaw no remedy for the firſt. I began, therefore, to deſpair of ultimate ſucceſs with tubes of porcelain *.

ANOTHER circumſtance confirmed me in this opinion. I found it impracticable to apply a heat above 27° to theſe tubes, when charged as above with carbonate, without deſtroying them, either by exploſion, by the formation of a minute rent, or by the actual ſwelling of the tube. Sometimes this ſwelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate ſuſtaining but a very ſmall loſs. This ductility of the porcelain in a low heat is a curious fact, and ſhews what a range of temperature is embraced by the gradual tranſition of ſome ſubſtances from a ſolid to a liquid ſtate: For the ſame porcelain, which is thus ſuſceptible of being ſtretched out without breaking in a heat of 27°, ſtands the heat of 152°, without injury, when expoſed to no violence, the angles of its fracture remaining ſharp and entire.

IV.

* I am neverthelefs of opinion, that, in ſome ſituations, experiments with compreſſion may be carried on with great eaſe and advantage in ſuch tubes. I allude to the ſituation of the geologiſts of France and Germany, who may eaſily procure, from their own manufactories, tubes of a quality far ſuperior to any thing made for ſale in this country.

IV.

Experiments in Gun-Barrels refumed.—The Vertical Apparatus applied to them.—Barrels bored in folid Bars.—Old Sable Iron.—Fufion of the Carbonate of Lime.—Its action on Porcelain.—Additional apparatus required in confequence of that action.—Good refults; in particular, four experiments, illustrating the theory of Internal Calcination, *and fhewing the efficacy of the Carbonic Acid as a Flux.*

Since I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expofe the carbonate in them to ftrong heats; I at laft determined to lay them afide, and return to barrels of iron, with which I had formerly obtained fome good refults, favoured, perhaps, by fome accidental circumftances.

On the 12th of February 1803, I began a feries of experiments with gun-barrels, refuming my former method of working with the fufible metal, and with lead; but altering the pofition of the barrel from horizontal to vertical; the breech being placed upwards during the action of heat on the carbonate. This very fimple improvement has been productive of advantages no lefs remarkable, than in the cafe of the tubes of porcelain. In this new pofition, the included air, quitting the air-tube on the fufion of the metal, and rifing to the breech, is expofed to the greateft heat of the furnace, and muft therefore react with its greateft force; whereas, in the horizontal pofition, that air might go as far back as the fufion of the metal reached, where its elafticity would be much feebler. The fame difpofition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a veffel filled with water; which contributed

contributed very much both to the convenience and ſafety of theſe experiments.

In this view, making uſe of the brick-furnace with the vertical muffle, already deſcribed in page 23. I ordered a pit (*a a a*, fig. 20.) to be excavated under it, for the purpoſe of receiving a water-veſſel. This veſſel (repreſented ſeparately, fig. 21.) was made of caſt iron; it was three inches in diameter, and three feet deep; and had a pipe (*d e*) ſtriking off from it at right angles, four or five inches below its rim, communicating with a cup (*e f*) at the diſtance of about two feet. The main veſſel being placed in the pit (*a a*) directly below the vertical muffle, and the cup ſtanding clear of the furnace, water poured into the cup flowed into the veſſel, and could thus conveniently be made to ſtand at any level. (The whole arrangement is repreſented in fig. 20.) The muzzle of the barrel (*g*) being plunged into the water, and its breech (*b*) reaching up into the muffle, as far as was found convenient, its poſition was ſecured by an iron chain (*g f*). The heat communicated downwards generally kept the ſurface of the water (at *c*) in a ſtate of ebullition; the waſte thus occaſioned being ſupplied by means of the cup, into which, if neceſſary, a conſtant ſtream could be made to flow.

As formerly, I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel would admit, in order to exclude as much of the fuſible metal as poſſible; for the expanſion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the leſs riſk there was of deſtroying the barrels.

In the courſe of practice, a ſimple mode occurred of removing the metal and withdrawing the cradle: it conſiſted in placing

cing the barrel with its muzzle downwards, ſo as to keep the breech above the furnace and cold, while its muzzle was expoſed to ſtrong heat in the muffle. In this manner, the metal was diſcharged from the muzzle, and the poſition of the barrel being lowered by degrees, the whole metal was removed in ſucceſſion, till at laſt the cradle and its contents became entirely looſe. As the metal was delivered, it was received in a crucible, filled with water, ſtanding on a plate of iron placed over the pit, which had been uſed, during the firſt ſtage of the experiment, to contain the water-veſſel. It was found to be of ſervice, eſpecially where lead was uſed, to give much more heat to the muzzle than ſimply what was required to liquefy the metal it contained; for when this was not done, the muzzle growing cold as the breech was heating, ſome of the metal delivered from the breech was congealed at the muzzle, ſo as to ſtop the paſſage.

According to this method, many experiments were made in gun-barrels, by which ſome very material ſteps were gained in the inveſtigation.

On the 24th February, I made an experiment with ſpar and chalk; the ſpar being placed neareſt to the breech of the barrel, and expoſed to the greateſt heat, ſome baked clay intervening between the carbonates. On opening the barrel, a long-continued hiſſing noiſe was heard. The ſpar was in a ſtate of entire calcination; the chalk, though crumbling at the outſide, was uncommonly hard and firm in the heart. The temperature had riſen to 32°.

In this experiment, we have the firſt clear example, in iron barrels, of what I call *Internal Calcination*; that is to ſay, where the carbonic acid ſeparated from the earthy baſis, has been accumulated in cavities within the barrel. For, ſubſequently to the action of ſtrong heat, the barrel had been completely cooled; the air therefore introduced by means of the air-tube, muſt have

have refumed its original bulk, and by itfelf could have no tendency to rufh out; the heat employed to open the barrel being barely fufficient to foften the metal. Since, then, the opening of the barrel was accompanied by the difcharge of elaftic matter in great abundance, it is evident, that this muft have proceeded from fomething fuperadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at leaft, internal; the feparation of the acid from the earthy matter being complete where the heat was ftrongeft, and only partial where the intenfity was lefs.

The chemical principles ftated in a former part of this paper, authorifed us to expect a refult of this kind. As heat, by increafing the volatility of the acid, tended to feparate it from the earth, we had reafon to expect, that, under the fame compreffion, but in different temperatures, one portion of the carbonate might be calcined, and another not: And that the leaft heated of the two, would be leaft expofed to a change not only from want of heat, but likewife in confequence of the calcination of the other mafs; for the carbonic acid difengaged by the calcination of the hotteft of the two, muft have added to the elafticity of the confined elaftic fluid, fo as to produce an increafe of compreffion. By this means, the calcination of the coldeft of the two might be altogether prevented, and that of the hotteft might be hindered from making any further advancement. This reafoning feemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened fome new practical views in thefe experiments, of which I availed myfelf without lofs of time. If the internal calcination of one part of an inclofed mafs, promotes the compreffion of other maffes included along with it, I conceived that we might forward our views very much by placing a fmall quantity of carbonate,

nate, carefully weighed, in the ſame barrel with a large quantity of that ſubſtance; and by arranging matters ſo that the ſmall fiducial part ſhould undergo a moderate heat, while a ſtronger heat, capable of producing internal calcination, ſhould be applied to the reſt of the carbonate. In this manner, I made many experiments, and obtained reſults which ſeemed to confirm this reaſoning, and which were often very ſatisfactory, though the heat did not always exert its greateſt force where I intended it to do ſo.

On the 28th of February, I introduced ſome carbonate, accurately weighed, into a ſmall porcelain tube, placed within a larger one, the reſt of the large tube being filled with pounded chalk; theſe carbonates, together with ſome pieces of chalk, placed along with the large tube in the cradle, weighing in all 195.7 grains. On opening the barrel, air ruſhed out with a long-continued hiſſing noiſe. The contents of the little tube were loſt by the intruſion of ſome borax which had been introduced over the ſilex, in order to exclude the fuſible metal. But the reſt of the carbonate, contained in the large tube, came out in a fine ſtate, being porous and frothy throughout; ſparkling every where with facettes, the angular form of which was diſtinguiſhable in ſome of the cavities by help of a lens: in ſome parts the ſubſtance exhibited the rounding of fuſion; in many it was in a high degree tranſparent. It was yellow towards the lower end, and at the other almoſt colourleſs. At the upper end, the carbonate ſeemed to have united with the tube, and at the places of contact to have ſpread upon it; the union having the appearance of a mutual action. The general maſs of carbonate efferveſced in acid violently, but the thin ſtratum immediately contiguous to the tube, feebly, if at all.

On the 3d of March, I introduced into a very clean tube of porcelain 36.8 of chalk. The tube was placed in the upper part of the cradle, the remaining ſpace being filled with two pieces of chalk, cut for the purpoſe; the uppermoſt of theſe being excavated, ſo as to anſwer the purpoſe of an air-tube. The pieces thus added, were computed to weigh about 300 grains. There was no pyrometer uſed; but the heat was gueſſed to be about 30°. After the barrel had ſtood during a few minutes in its delivering poſition, the whole lead, with the rod and cradle, were thrown out with a ſmart report, and with conſiderable force. The lowermoſt piece of chalk had ſcarcely been acted upon by heat. The upper part of the other piece was in a ſtate of marble, with ſome remarkable facettes. The carbonate, in the little tube, had ſhrunk very much during the firſt action of heat, and had begun to ſink upon itſelf, by a further advancement towards liquefaction. The maſs was divided into ſeveral cylinders, lying confuſedly upon each other; this diviſion ariſing from the manner in which the pounded chalk was rammed into the tube in ſucceſſive portions. In ſeveral places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a ſoft ſubſtance; the globular form and ſhining ſurface of all theſe cavities, clearly indicating fuſion. The ſubſtance was ſemitranſparent; in ſome places yellow, and in ſome colourleſs. When broken, the ſolid parts ſhewed a ſaline fracture, compoſed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, ſo as to render it impoſſible to aſcertain what loſs had been ſuſtained. In general, the line of contact was of a brown colour; yet there was no room for ſuſpecting the preſence of any foreign matter, except, perhaps, from the iron-rod which was uſed in ramming down the chalk. But, in ſubſequent experiments, I have obſerved the ſame brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purpoſely rammed with a piece of wood;

wood; fo that this colour, which has occurred in almoft every fimilar cafe, remains to be accounted for. The carbonate effervefced violently with acid; the fubftance in contact with the tube, doing fo, however, more feebly than in the heart, leaving a copious depofite of white fandy matter, which is doubtlefs a part of the tube, taken up by the carbonate in fufion.

On the 24th of March, I made a fimilar experiment, in a ftout gun-barrel, and took fome care, after the application of heat, to cool the barrel flowly, with a view to cryftallization. The whole mafs was found in a fine ftate, and untouched by the lead; having a femitranfparent and faline ftructure, with various facettes. In one part, I found the moft decided cryftallization I had obtained, though of a fmall fize: owing to its tranfparency it was not eafily vifible, till the light was made to reflect from the cryftalline furface, which then produced a dazzle, very obfervable by the naked eye: when examined by means of a lens, it was feen to be compofed of feveral plates, broken irregularly in the fracture of the fpecimen, all of which are parallel to each other, and reflect under the fame angle, fo as to unite in producing the dazzle. This ftructure was obfervable equally well in both parts of the broken fpecimen. In a former experiment, as large a facette was obtained in a piece of folid chalk; but this refult was of more confequence, as having been produced from chalk previoufly pounded.

The foregoing experiments proved the fuperior efficacy of iron veffels over thofe of porcelain, even where the thicknefs was not great; and I perfevered in making a great many experiments with gun-barrels, by which I occafionally obtained very fine refults: but I was at laft convinced, that their thicknefs was not fufficient to enfure regular and fteady fuccefs: For this purpofe, it appeared proper to employ veffels of fuch ftrength, as to bear a greater expanfive force than was juft neceffary; fince, occafionally, (owing to our ignorance of the relation between the various forces of expanſion, affinity, tenacity, &c.), much more ſtrain has been given to the veſſels than was requiſite. In ſuch caſes, barrels have been deſtroyed, which, as the reſults have proved, had acted with ſufficient ſtrength during the firſt ſtages of the experiments, though they had been unable to reſiſt the ſubſequent overſtrain. Thus, my ſucceſs with gun-barrels, depended on the good fortune of having uſed a force no more than ſufficient, to conſtrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourſe to iron barrels of much greater ſtrength, and tried various modes of conſtruction.

I HAD ſome barrels executed by wrapping a thick plate of iron round a mandrel, as is practiſed in the formation of gun-barrels; and likewiſe by bringing the two flat ſides together, ſo as to unite them by welding. Theſe attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the ſolid, ſo as to form a barrel. In this manner I ſucceeded well. The firſt barrel I tried in this way was of ſmall bore, only half an inch: Its performance was highly ſatisfactory, and ſuch as to convince me, that the mode now adopted was the beſt of any that I had tried. Owing to the ſmallneſs of the bore, a pyrometer could not be uſed internally, but was placed upon the breech of the barrel, as it ſtood in the vertical muffle. In this poſition, it was evidently expoſed to a much leſs heat than the fiducial part of the apparatus, which was always placed, as nearly as could be gueſſed, at the point of greateſt heat.

ON the 4th of April, an experiment was made in this way with ſome ſpar; the pyrometer on the breech giving 33°. The ſpar came out clean, and free from any contamination, adhering to the inſide of the porcelain tube: it was very much ſhrunk, ſtill retaining a cylindrical form, though bent by partial adheſions. Its ſurface bore ſcarcely any remains of the impreſſion taken by the

the powder, on ramming it into the tube: it had, to the naked eye, the roughneſs and ſemitranſparency of the pith of a ruſh ſtripped of its outer ſkin. By the lens, this ſame ſurface was ſeen to be glazed all over, though irregularly, ſhewing here and there ſome air-holes. In fracture, it was ſemitranſparent, more vitreous than cryſtalline, though having a few facettes: the maſs, was ſeemingly formed of a congeries of parts, in themſelves quite tranſparent: and, at the thin edges, ſmall pieces were viſible of perfect tranſparency. Theſe muſt have been produced in the fire; for the ſpar had been ground with water, and paſſed through ſieves, the ſame with the fineſt of thoſe uſed at Etruria, as deſcribed by Mr WEDGWOOD, in his paper on the conſtruction of his Pyrometer.

WITH the ſame barrel I obtained many intereſting reſults, giving as ſtrong proofs of fuſion as in any former experiments; with this remarkable difference, that, in theſe laſt, the ſubſtance was compact, with little or no trace of frothing. In the gun-barrels where fuſion had taken place, there had always been a loſs of 4 or 5 *per cent.*, connected, probably, with the frothing. In theſe experiments, for a reaſon ſoon to be ſtated, the circumſtance of weight could not be obſerved; but appearances led me to ſuppoſe, that here the loſs had been ſmall, if any.

ON the 6th of April, I made another experiment with the ſquare barrel, whoſe thickneſs was now much reduced by ſucceſſive ſcales, produced by oxidation, and in which a ſmall rent began to appear externally, which did not, however, penetrate to the bore. The heat roſe high, a pyrometer on the breech of the barrel giving 37°. On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident, that the barrel had not been laid open, for part of the carbonate was in a ſtate of ſaline

marble;

marble; another was hard and white, without any faline grains, and fcarcely effervefced in acid. It was probably quicklime, formed by internal calcination, but in a ftate that has not occurred in any other experiment.

The workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was feen in this piece, I was furprifed to fee the infide of the barrel lined with a fet of tranfparent and well-defined cryftals, of fmall fize, yet vifible by the naked eye. They lay together in fome places, fo as to cover the furface of the iron with a tranfparent coat; in others they were detached, and fcattered over the furface. Unfortunately, the quantity of this fubftance was too fmall to admit of much chemical examination; but I immediately afcertained, that it did not in the leaft effervefce in acid, nor did it feem to diffolve in it. The cryftals were in general tranfparent and colourlefs, though a few of them were tinged feemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a ftrong refemblance to the cryftals of the Lamellated Stylbite of Haüy. Though made above two years ago, they ftill retain their form and tranfparency unchanged. Whatever this fubftance may be, its appearance, in this experiment, is in the higheft degree interefting, as it feems to afford an example of the mode in which Dr Hutton fuppofes many internal cavities to have been lined, by the fublimation of fubftances in a ftate of vapour; or, held in folution, by matters in a gafeous form. For, as the cryftals adhered to a part of the barrel, which muft have been occupied by air during the action of heat, it feems next to certain that they were produced by fublimation.

The very powerful effects produced by this laft barrel, the fize of which (reduced, indeed, by repeated oxidation) was not above

above an inch ſquare, made me very anxious to obtain barrels of the ſame ſubſtance, which being made of greater ſize, ought to afford reſults of extreme intereſt. I found upon inquiry, that this barrel was not made of Swediſh iron, as I at firſt ſuppoſed, but of what is known by the name of *Old Sable*, from the figure of a Sable ſtamped upon the bars; that being the armorial badge of the place in Siberia where this iron is made *.

A WORKMAN explained to me ſome of the properties of different kinds of irons, moſt intereſting in my preſent purſuit; and he illuſtrated what he ſaid by actual trial. All iron, when expoſed to a certain heat, cruſhes and crumbles under the hammer; but the temperature in which this happens, varies with every different ſpecies. Thus, as he ſhewed me, caſt iron cruſhes in a dull-red heat, or perhaps about 15° of WEDGWOOD; ſteel, in a heat perhaps of 30°; Swediſh iron, in a bright white heat, perhaps of 50° or 60°; old ſable, itſelf, likewiſe yields, but in a much higher heat, perhaps of 100°. I merely gueſſed at theſe temperatures; but I am certain of this, that in a heat ſimilar to that in which Swediſh iron crumbled under the hammer, the old ſable withſtood a ſtrong blow, and ſeemed to poſſeſs conſiderable firmneſs. It is from a knowledge of this quality, that the blackſmith, when he firſt takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has ſunk to the degree in which the iron can bear the hammer. I obſerved, as the ſtrong heat of the forge acted on the Swediſh iron, that it began to boil at the ſurface, clearly indicating the diſcharge of ſome gaſeous matter; whereas, the old ſable, in the ſame circumſtances, acquired the ſhining ſurface of a liquid, and melted away without any effervеſcence. I procured, at this time, a conſiderable number

* I WAS favoured with this account of it by the late Profeſſor ROBISON.

number of bars of that iron, which fully anſwered my expectations.

By the experiments laſt mentioned, a very important point was gained in this inveſtigation; the complete fuſibility of the carbonate under preſſure being thereby eſtabliſhed. But from this very circumſtance, a neceſſity aroſe of adding ſome new devices to thoſe already deſcribed: for the carbonate, in fuſion, ſpreading itſelf on the inſide of the tube containing it, and the two uniting firmly together, ſo as to be quite inſeparable, it was impoſſible, after the experiment, to aſcertain the weight of the carbonate by any method previouſly uſed. I therefore determined in future to adopt the following arrangement.

A small tube of porcelain (*i k*, fig. 23.) was weighed by means of a counterpoiſe of ſand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment, was aſcertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had taken place between the tube and the carbonate. The balance which I uſed, turned, in a conſtant and ſteady manner, with one hundredth of a grain. When pounded chalk was rammed into this tube, I generally left part of it free, and in that ſpace laid a ſmall piece of lump-chalk (*i*), dreſſed to a cylinder, with the ends cut flat and ſmooth, and I uſually cut a letter on each end, the more effectually to obſerve the effects produced by heat upon the chalk; the weight of this piece of chalk being always eſtimated along with that of the powder contained in the tube. In ſome experiments, I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it), in order to provide againſt the caſe of ebullition:

ebullition: But as that did not often occur, I ſeldom took the trouble of this laſt precaution.

It was now of conſequence to protect the tube, thus prepared, from being touched during the experiment, by any ſubſtance, above all, by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The ſmall tube (Fig. 23. *i k*), with its pounded carbonate (*k*), and its cylinder of lump-chalk (*i*,), was dropt into a large tube of porcelain (*p k*, Fig. 24.). Upon this a fragment of porcelain (*l*), of ſuch a ſize as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dreſſed, ſo as nearly to fit and fill up the inſide of the large tube, one end of it being rudely cut into the form of a cone. This maſs being then introduced, with its cylindrical end downwards, was made to preſs upon the fragment of porcelain (*l*). I then dropped into the ſpace (*n*), between the conical part of this maſs and the tube, a ſet of fragments of chalk, of a ſize beyond what could poſſibly fall between the cylindrical part and the tube, and preſſed them down with a blunt tool, by which the chalk being at the ſame time cruſhed and rammed into the angle, was forced into a maſs of ſome ſolidity, which effectually prevented any thing from paſſing between the large maſs of chalk and the tube. In practice, I have found this method always to anſwer, when done with care. I covered the chalk, thus rammed, with a ſtratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner, I filled the whole of the large tube with alternate layers of ſilex and chalk; the muzzle being always occupied with chalk, which was eaſily preſſed into a maſs of tolerable firmneſs, and, ſuffering no change in very low heats, excluded the fuſible metal in the firſt ſtages of the experiment.

The large tube, thus filled, was placed in the cradle, ſometimes with the muzzle upwards, and ſometimes the reverſe. I have frequently altered my views as to that part of the arrangement, each mode poſſeſſing peculiar advantages and diſadvantages. With the muzzle upwards, (as ſhewn in fig. 24. and 25.) the beſt ſecurity is afforded againſt the intruſion of the fuſible metal; becauſe the air, quitting the air-tube in the working poſition, occupies the upper part of the barrel; and the fuſible metal ſtands as a liquid (at *q*, fig. 25.) below the muzzle of the tube, ſo that all communication is cut off, between the liquid metal and the inſide of the tube. On the other hand, by this arrangement, the ſmall tube, which is the fiducial part of the apparatus, is placed at a conſiderable diſtance from the breech of the barrel, ſo as either to undergo leſs heat than the upper part, or to render it neceſſary that the barrel be thruſt high into the muffle.

With the muzzle of the large tube downwards, the inner tube is placed (as ſhewn in fig. 22.), ſo as ſtill to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the ſmall tube near to the breech of the barrel: and though there is here leſs ſecurity againſt the intruſion of liquid metal, I have found that a point of little conſequence; ſince, when the experiment is a good one, and that the carbonic acid has been well confined, the intruſion ſeldom takes place in any poſition. In whichever of the two oppoſite poſitions the large tube was placed, a pyrometer was always introduced, ſo as to lie as near as poſſible to the ſmall tube. Thus, in the firſt-mentioned poſition, the pyrometer was placed immediately below the large tube, and, in the other poſition, above it; ſo that, in both caſes, it was ſeparated from the carbonate by the thickneſs only of the two tubes.

Much room was unavoidably occupied by this method, which neceſſarily obliged me to uſe ſmall quantities of carbonate,

bonate, the ſubject of experiment ſeldom weighing more than 10 or 12 grains, and in others far leſs *.

On the 11th of April 1803, with a barrel of old ſable iron having a bore of 0.75 of an inch, I made an experiment in which all theſe arrangements were put in practice. The large tube contained two ſmall ones ; one filled with ſpar, and the other with chalk. I conceived that the heat had riſen to 33°, or ſomewhat higher. On melting the metals, the cradle was thrown out with conſiderable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my aſtoniſhment, indicated 64°. Yet all was ſound. The two little tubes came out quite clean and uncontaminated. The ſpar had loſt 17.0 *per cent.*: The chalk 10.7 *per cent.*: The ſpar was half ſunk down, and run againſt the ſide of the little tube: Its ſurface was ſhining, its texture ſpongy, and it was compoſed of a tranſparent and jelly-like ſubſtance : The chalk was entirely in a ſtate of froth. This experiment extends our power of action, by ſhewing, that compreſſion, to a conſiderable degree, can be carried on in ſo great a heat as 64°. It ſeems likewiſe to prove, that, in ſome of the late experiments with the ſquare barrel, the heat had been much higher than was ſuppoſed at the time, from the indication of the pyrometer placed on the breech of the barrel ; and that in ſome of them, particularly in the laſt, it muſt have riſen at leaſt as high as in the preſent experiment.

* I meaſured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal ſand. By comparing the weight of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330.6 grains, and that each grain of it correſponded to 0.00075 of a cubic inch. From theſe data I was able, with tolerable accuracy, to gage a tube by weighing the tin required to fill it.

On the 21ſt of April 1805, a ſimilar experiment was made with a new barrel, bored in a ſquare bar of old ſable, of about two and a half inch in diameter, having its angles merely rounded; the inner tube being filled with chalk. The heat was maintained during ſeveral hours, and the furnace allowed to burn out during the night. The barrel had the appearance of ſoundneſs, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63°. On examination, and after beating off the ſmooth and even ſcale of oxide peculiar to the old ſable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithſtanding the failure of the barrel, was one of the moſt intereſting I had made, ſince it afforded proof of complete fuſion. The carbonate had boiled over the lips of the little tube, ſtanding, as juſt deſcribed, with its mouth upwards, and had run down to within half an inch of its lower end: moſt of the ſubſtance was in a frothy ſtate, with large round cavities, and a ſhining ſurface; in other parts, it was interſperſed with angular maſſes, which have evidently been ſurrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no efferveſcence, and not turning red like quicklime in nitric acid, which ſeemed to have no effect upon it in the lump. It was probably a compound of quicklime with the ſubſtance of the tube.

With the ſame barrel repaired, and with others like it, many ſimilar experiments were made at this time with great ſucceſs; but to mention them in detail, would amount nearly to a repetition of what has been ſaid. I ſhall take notice of only four of them, which, when compared together, throw much light on the theory of theſe operations, and likewiſe ſeem to eſtabliſh a very important principle in geology. Theſe four

four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

The firſt of theſe experiments was made on the 27th of April 1803, in one of the large barrels of old ſable, with all the above-mentioned arrangements. The heat had riſen, contrary to my intention, to 78° and 79°. The tubes came out uncontaminated with fuſible metal, and every thing bore the appearance of ſoundneſs. The contents of the little tube, conſiſting of pounded chalk, and of a ſmall piece of lump-chalk, came out clean, and quite looſe, not having adhered to the inſide of the tube in the ſmalleſt degree. There was a loſs of 41 *per cent.*, and the calcination ſeemed to be complete; the ſubſtance, when thrown into nitric acid, turning red, without effervefcence at firſt, though, after lying a few minutes, ſome bubbles appeared. According to the method followed in all theſe experiments, and lately deſcribed at length, (and ſhewn in fig. 24. & 25.), the large tube was filled over the ſmall one, with various maſſes of chalk, ſome in lump, and ſome rammed into it in powder; and in the cradle there lay ſome pieces of chalk, filling up the ſpace, ſo that in the cradle there was a continued chain of carbonate of four or five inches in length. The ſubſtance was found to be leſs and leſs calcined, the more it was removed from the breech of the barrel, where the heat was greateſt. A ſmall piece of chalk, placed at the diſtance of half an inch from the ſmall tube, had ſome ſaline ſubſtance in the heart, ſurrounded and intermixed with quicklime, diſtinguiſhed by its dull white. In nitric acid, this ſubſtance became red, but effervefced pretty briſkly; the effervefcence continuing till the whole was diſſolved. The next portion of chalk, was in a firm ſtate of limeſtone; and a lump of chalk in the cradle, was equal in perfection to any marble I have obtained by compreſſion: the two laſt-mentioned pieces of chalk effervefcing with violence in the acid, and ſhewing

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no rednefs when thrown into it. Thefe facts clearly prove, that the calcination of the contents of the fmall tube had been internal, owing to the violent heat which had feparated its acid from the moft heated part of the carbonate, according to the theory already ftated. The foundnefs of the barrel was proved by the complete ftate of thofe carbonates which lay in lefs heated parts. The air-tube in this experiment had a capacity of 0.29, nearly one-third of a cubic inch.

The fecond of thefe experiments was made on the 29th of April, in the fame barrel with the laft, after it had afforded fome good refults. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch. The heat rofe to 60°. The barrel was covered externally with a black fpongy fubftance, the conftant indication of failure, and a fmall drop of white metal made its appearance. The cradle was removed without any explofion or hiffing. The carbonates were entirely calcined. The barrel had yielded, but had refifted well at firft; for, the contents of the little tube were found in a complete ftate of froth, and running with the porcelain.

The third experiment was made on the 30th of April, in another fimilar barrel. Every circumftance was the fame as in the two laft experiments, only that the air-tube was now reduced to half its laft bulk, that is, to one-twentieth of a cubic inch. A pyrometer was placed at each end of the large tube. The uppermoft gave 41°, the other only 15°. The contents of the inner tube had loft 16 *per cent.*, and were reduced to a moft beautiful ftate of froth, not very much injured by the internal calcination, and indicating a thinner ftate of fufion than had appeared.

The fourth experiment was made on the 2d of May, like the reft in all refpects, with a ftill fmaller air-tube, of 0.0318, being lefs than one-thirtieth of a cubic inch. The upper pyrometer

rometer gave 25°, and the under one 16° : The loweſt maſſes of carbonate were ſcarcely affected by the heat : The contents of the little tube had loſt 2.9 *per cent.* ; both the lump and the pounded chalk were in a fine ſaline ſtate, and, in ſeveral places had run and ſpread upon the inſide of the tube, which I had not expected to ſee in ſuch a low heat. On the upper ſurface of the chalk rammed into the little tube, which, after its introduction had been wiped ſmooth, were a ſet of white cryſtals, with ſhining facettes, large enough to be diſtinguiſhed by the naked eye, and ſeeming to riſe out of the maſs of carbonate. I likewiſe obſerved, that the ſolid maſs on which theſe cryſtals ſtood, was uncommonly tranſparent.

In theſe four experiments, the bulk of the included air was ſucceſſively diminiſhed, and by that means its elaſticity increaſed. The conſequence was, that in the firſt experiment, where that elaſticity was the leaſt, the carbonic acid was allowed to ſeparate from the lime, in an early ſtage of the riſing heat, lower than the fuſing point of the carbonate, and complete internal calcination was effected. In the ſecond experiment, the elaſtic force being much greater, calcination was prevented, till the heat roſe ſo high as to occaſion the entire fuſion of the carbonate, and its action on the tube, before the carbonic acid was ſet at liberty by the failure of the barrel. In the third experiment, with ſtill greater elaſtic force, the carbonate was partly conſtrained, and its fuſion accompliſhed, in a heat between 41° and 15°. In the laſt experiment, where the force was ſtrongeſt of all, the carbonate was almoſt completely protected from decompoſition by heat, in conſequence of which it cryſtallized and acted on the tube, in a temperature between 25° and 16°. On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced ; ſince the firſt experiment

periment proved, that quicklime by itſelf, could neither be melted, nor act upon porcelain, even in the violent heat of 79°; whereas, in the laſt experiment where the carbonic acid was retained, both of theſe effects took place in a very low temperature.

V.

Experiments in which Water was employed to increaſe the Elaſticity of the included Air.—Caſes of complete Compreſſion.—General Obſervations.—Some Experiments affording intereſting reſults; in particular, ſhewing a mutual action between Silex and the Carbonate of Lime.

FINDING that ſuch benefit aroſe from the increaſe of elaſticity given to the included air in the laſt-mentioned experiments, by the diminution of its quantity; it now occurred to me, that a ſuggeſtion formerly made by Dr KENNEDY, of uſing water to aſſiſt the compreſſing force, might be followed with advantage: That while ſufficient room was allowed for the expanſion of the liquid metal, a reacting force of any required amount, might thus be applied to the carbonate. In this view, I adopted the following mode, which, though attended with conſiderable difficulty in execution, I have often practiſed with ſucceſs. The weight of water required to be introduced into the barrel was added to a ſmall piece of chalk or baked clay, previouſly weighed. The piece was then dropped into a tube of porcelain of about an inch in depth, and covered with pounded chalk, which was firmly rammed upon it. The tube was then placed in the cradle along with the ſubject of experiment, and the whole was plunged into the fuſible metal, previouſly poured into the barrel, and heated ſo as merely to render it liquid. The metal being thus ſuddenly cooled, the

the tube was encafed in a folid mafs, before the heat had reached the included moifture. The difficulty was to catch the fufible metal at the proper temperature; for when it was fo hot as not to fix in a few feconds, by the contact of the cradle and its contents, the water was heard to bubble through the metal and efcape. I overcame this difficulty, however, by firft heating the breech of the barrel, (containing a fufficient quantity of fufible metal), almoft to rednefs, and then fetting it into a veffel full of water, till the temperature had funk to the proper pitch, which I knew to be the cafe when the hiffing noife produced in the water by the heated barrel ceafed; the cradle, during the laft ftage of this operation, being held clofe to the muzzle of the barrel, and ready to be thruft into it.

On the 2d of May, I made my firft experiment in this way, ufing the fame air-tube as in the laft experiment, which was equal in capacity to one-thirtieth of a cubic inch. Half a grain of water was introduced in the manner juft defcribed. The barrel, after an hour of red-heat, was let down by a rope and pulley, which I took care to ufe in all experiments, in which there was any appearance of danger. All was found. The metals rufhed out fmartly, and a flafh of flame accompanied the difcharge. The upper pyrometer gave 24°, and the lower one 14°. The contents of the inner tube had loft lefs than 1 *per cent.*, ftrictly 0.84. The carbonate was in a ftate of good limeftone; but the heat had been too feeble: The lower part of the chalk in the little tube was not agglutinated: The chalk round the fragment of pipe-ftalk (ufed to introduce the water), which had been more heated than the pyrometer, and the fmall rod, which had moulded itfelf in the boll of the ftalk, were in a ftate of marble.

On the 4th of May, I made an experiment like the laft, but with the addition of 1.05 grains water. After application of heat, the fire was allowed to burn out till the barrel was black. The metal was difcharged irregularly. Towards the end, the inflammable air produced, burnt at the muzzle, with a lambent flame; during fome time, arifing doubtlefs from hydrogen gas, more or lefs pure, produced by the decompofition of the water. The upper pyrometer indicated 36°, and the lower one 19°. The chalk which lay in the outer part of the large tube was in a ftate of marble. The inner tube was united to the outer one, by a ftar of fufed matter, black at the edges, and fpreading all round, furrounding one of the fragments of porcelain which had fallen by accident in between the tubes. The inner tube, with the ftarry matter adhering to it, but without the coated fragment, feemed to have fuftained a lofs of 12 *per cent.*, on the original carbonate introduced. But, the fubftance furrounding the fragment being inappreciable, it was impoffible to learn what lofs had been really fuftained. Examining the little tube, I found its edges clean, no boiling over having taken place. The top of the fmall lump of chalk had funk much. When the little tube was broken, its contents gave proof of fufion in fome parts, and in others, of the neareft approach to it. A ftrong action of ebullition had taken place all round, at the contact of the tube with the carbonate: in the heart, the fubftance had a tranfparent granular texture, with little or no cryftallization. The fmall piece of lump-chalk was united and blended with the rammed powder, fo that they could fcarcely be diftinguifhed. In the lower part of the carbonate, where the heat muft have been weaker, the rod had acted more feebly on the tube, and was detached from it: here the fubftance was firm, and was highly marked in the fracture with cryftalline facettes. Wherever the carbonate touched the tube, the two fubftances exhibited, in their mixture, much greater proofs of fufion than could be found in the pure carbonate. At one place, a ftream of this compound had penetrated a rent in the

the inner tube, which it had filled completely, conſtituting a real vein, like thoſe of the mineral kingdom: which is ſtill diſtinctly to be ſeen in the ſpecimen. It had then ſpread itſelf upon the outſide of the inner tube, to the extent of half an inch in diameter, and had enveloped the fragment of porcelain already mentioned. When pieces of the compound were thrown into nitric acid, ſome effervefced, and ſome not.

I REPEATED this experiment on the ſame day, with two grains of water. The furnace being previouſly hot; I continued the fire during one half-hour with the muffle open, and another with a cover upon it. I then let the barrel down by means of the pulley. The appearance of a large longitudinal rent, made me at firſt conceive that the experiment was loſt, and the barrel deſtroyed: The barrel was viſibly ſwelled, and in ſwelling had burſt the cruſt of ſmooth oxide with which it was ſurrounded; at the ſame time, no exudation of metal had happened, and all was ſound. The metals were thrown out with more ſuddenneſs and violence than in any former experiment, but the rod remained in its place, being ſecured by a cord. The upper pyrometer gave 27°, the lower 23°. The contents of the inner tube had loſt 1.5 *per cent.* The upper end of the little lump of chalk, was rounded and glazed by fuſion; and the letter which I have been in the habit of cutting on theſe ſmall pieces, in order to trace the degree of action upon them, was thus quite obliterated. On the lower end of the ſame lump, the letter is ſtill viſible. Both the lump and the rammed chalk were in a good ſemitranſparent ſtate, ſhining a little in the fracture, but with no good facettes, and no where appearing to have acted on the tube. This laſt circumſtance is of conſequence, ſince it ſeems to ſhew, that this very remarkable action of heat, under compreſſion, was performed without the aſſiſtance of the ſubſtance of the tube, by which, in many other experiments, a confiderable additional fufibility has been communicated to the carbonate.

These experiments, and many others made about the fame time, with the fame fuccefs, clearly prove the efficacy of water in affifting the compreffion; and refults approaching to thefe in quality, obtained, in fome cafes, by means of a very fmall air-tube, fhew that the influence of water on this occafion has been merely mechanical.

During the following fummer and autumn 1803, I was occupied with a different branch of this fubject, which I fhall foon have occafion to mention.

In the early part of laft year, 1804, I again refumed the fort of experiments lately defcribed, having in view principally to accomplifh abfolute compreffion, in complete imitation of the natural procefs. In this purfuit, I did not confine myfelf to water, but made ufe of various other volatile fubftances, in order to affift compreffion; namely, carbonate of ammonia, nitrate of ammonia, gunpowder, and paper impregnated with nitre. With thefe I obtained fome good refults, but none fuch as to induce me to prefer any of thefe compreffors to water. Indeed, I am convinced, that water is fuperior to them all. I found, in feveral experiments, made with a fimple air-tube, without any artificial compreffor, in which a very low red-heat had been applied, that the carbonate loft one or one and a half *per cent*. Now, as this muft have happened in a temperature fcarcely capable of inflaming gunpowder, it is clear, that fuch lofs would not have been prevented by its prefence: whereas water, beginning far below rednefs to affume a gafeous form, will effectually refift any calcination, in low as well as in high heats. And as the quantity of water can very eafily be regulated by weight, its employment for this purpofe feems liable to no objection.

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On the 2d of January 1804, I made an experiment with marble and chalk, with the addition of 1.1 grain of water. I aimed at a low heat, and the pyrometer, though a little broken, feemed clearly to indicate 22°. Unluckily, the muzzle of the large tube, which was clofed as ufual with chalk, was placed uppermoft, and expofed to the ftrongeft heat. I found it rounded by fufion, and in a frothy ftate. The little tube came out very clean, and was fo nearly of the fame weight as when put in, that its contents had loft but 0.074 *per cent.* of the weight of the original carbonate. The marble was but feebly agglutinated, but the chalk was in a ftate of firm limeftone, though it muft have undergone a heat under 22°, or that of melting filver. This experiment is certainly a moft remarkable one, fince a heat has been applied, in which the chalk has been changed to hard limeftone, with a lofs lefs than the 1000dth par of its weight, (exactly $\frac{1}{1351}$) ; while, under the fame circumftances of preffure, though probably with more heat, fome of the fame fubftance had been brought to fufion. What lofs of weight this fufed part fuftained, cannot be known.

On the 4th of January, a fimilar experiment was made, likewife with 1.1 grain of water. The difcharge of the metal was accompanied with a flafh of flame. The pyrometer indicated 26°. The little tube came out quite clean. Its contents had been reduced from 14.53 to 14.46, difference 0.07 grains, being 0.47 *per cent.* on the original carbonate, lefs than one two-hundredth part of the original weight, (exactly $\frac{1}{212}$). The chalk was in a ftate of firm faline marble, but with no unufual qualities.

These two laft experiments are rendered ftill more interefting, by another fet which I made foon after, which fhewed, that one effential precaution in a point of fuch nicety had been neglected, in not previoufly drying the carbonate. In feveral trials made in the latter end of the fame month,

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I found, that chalk expofed to a heat above that of boiling water, but quite fhort of rednefs, loft 0.34 *per cent.*; and in another fimilar trial, 0.46 *per cent.* Now, this lofs of weight equals within 0.01 *per cent.* the lofs in the laft-mentioned experiment, that being 0.47; and far furpaffes that of the laft but one, which was but 0.074. There is good reafon, therefore, to believe, that had the carbonate, in thefe two laft experiments, been previoufly dried, it would have been found during compreffion to have undergone no lofs.

The refult of many of the experiments lately mentioned, feems fully to explain the perplexing difcordance between my experiments with porcelain tubes, and thofe made in barrels of iron. With the procelain tubes, I never could fucceed in a heat above 28°, or even quite up to it; yet the refults were often excellent. Whereas, the iron-barrels have currently ftood firm in heats of 41° or 51°, and have reached even to 70° or 80° without injury. At the fame time, the refults, even in thofe high heats, were often inferior, in point of fufion, to thofe obtained by low heats in porcelain. The reafon of this now plainly appears. In the iron-barrels it has always been confidered as neceffary to ufe an air-tube, in confequence of which, fome of the carbonic acid has been feparated from the earthy bafis by internal calcination: what carbonic acid remained, has been more forcibly attracted, according to M. Berthollet's principle, and, of courfe, more eafily compreffed, than when of quantity fufficient to faturate the lime: but, owing to the diminifhed quantity of the acid, the compound has become lefs fufible than in the natural ftate, and, of courfe, has undergone a higher heat with lefs effect. The introduction of water, by furnifhing a reacting force, has produced a ftate of things fimilar to that in the porcelain tubes; the carbonate fuftaining little or no lofs of weight,

weight, and the compound retaining its fufibility in low heats *.

In the early part of 1804, fome experiments were made with barrels, which I wifhed to try, with a view to another feries of experiments. The refults were too interefting to be paffed over; for, though the carbonic acid in them was far from being completely conftrained, they afforded fome of the fineft examples I had obtained, of the fufion of the carbonate, and of its union with filex.

On the 13th of February, an experiment was made with pounded oyfter-fhell, in a heat of 33°, without any water being introduced to affift compreffion. The lofs was apparently of 12 *per cent.* The fubftance of the fhell had evidently been in vifcid fufion: it was porous, femitranfparent, fhining in furface and fracture; in moft parts with the glofs of fufion, in many others with facettes of cryftallization. The little tube had been fet with its muzzle upwards; over it, as ufual, lay a fragment of porcelain, and on that a round mafs of chalk. At the contact of the porcelain and the chalk, they had run together, and the chalk had been evidently in a very foft ftate; for, refting with its weight on the porcelain, this laft had been preffed into the fubftance of the chalk, deeper than its own breadth, a rim of chalk being vifible without the furface of the porcelain; juft as when the round end of a knife is preffed upon

* The retentive power here afcribed to the procelain tubes, feems not to accord with what was formerly mentioned, of the carbonic acid having been driven through the fubftance of the tube. But the lofs by this means has probably been fo fmall, that the native properties of the carbonate have not been fenfibly changed. Or, perhaps, this penetrability may not be fo univerfal as I have been induced to think, by having met with it in all the cafes which I tried. In this doubt, I ftrenuoufly recommend a further examination of this fubject to gentlemen who have eafy accefs to fuch procelains as that of Drefden or of Seve.

upon a piece of ſoft butter. The carbonate had ſpread very much on the inſide of the tube, and had riſen round its lip, as ſome ſalts riſe from their ſolution in water. In this manner, a ſmall quantity of the carbonate had reached the outer tube, and had adhered to it. The black colour frequently mentioned as accompanying the union of the carbonates with the porcelain, is here very remarkable.

On the 26th of February, I made an experiment, in which the carbonate was not weighed, and no foreign ſubſtance was introduced to aſſiſt the compreſſion. The temperature was 46°. The pyrometer had been affected by the contact of a piece of chalk, with which it had united; and ſome of the carbonate muſt have penetrated the ſubſtance of the pyrometer, ſince this laſt had viſibly yielded to preſſure, as appeared by a ſwelling near the contact. I obſerved in theſe experiments, that the carbonate had a powerful action on the tubes of Corniſh clay, more than on the pounded ſilex. Perhaps it has a peculiar affinity for argil, and this may lead to important conſequences. The chalk had viſibly firſt ſhrunk upon itſelf, ſo as to be detached from the ſides, and had then begun to run by ſucceſſive portions, ſo as ſtill to leave a pillar in the middle, very irregularly worn away; indicating a ſucceſſive liquefaction, like that of ice, not the yielding of a maſs ſoftening all at once.

On the 28th of February, I made an experiment with oyſter-ſhell unweighed, finely ground, and paſſed through the cloſeſt ſieves. The pyrometer gave 40°. The piece of chalk below it had been ſo ſoft, as to ſink to the depth of half an inch into the mouth of the iron air-tube, taking its impreſſion completely. A ſmall part of this lump was contaminated with iron, but the reſt was in a fine ſtate. The tube had a rent in it, through which the carbonate, united with the matter of the tube, had flowed in two or three places. The

ſhell

ſhell had ſhrunk upon itſelf, ſo as to ſtand detached from the ſides, and bore very ſtrong marks of fuſion. The external ſurface was quite ſmooth, and ſhining like an enamel. The internal part conſiſted of a mixture of large bubbles and ſolid parts: the inſide of the bubbles had a luſtre much ſuperior to that of the outſide, and equal to that of glaſs. The general maſs was ſemitranſparent; but ſmall parts were viſible by the lens, which were completely tranſparent and colourleſs. In ſeveral places this ſmooth ſurface had cryſtallized, ſo as to preſent brilliant facettes, ſteadily ſhining in certain aſpects. I obſerved one of theſe facettes on the inſide of an air-bubble, in which it interrupted the ſpherical form as if the little ſphere had been preſſed inwards at that ſpot, by the contact of a plane ſurface. In ſome chalk near the mouth of the large tube, which lay upon a ſtratum of ſilex, another very intereſting circumſtance occurred. Connected with its lower end, a ſubſtance was viſible, which had undoubtedly reſulted from the union of the carbonate with the ſilex. This ſubſtance was white and ſemitranſparent, and bore the appearance of chalcedony. The maſs of chalk having attached itſelf to that above it, had ſhrunk upwards, leaving an interval between it and the ſilex, and carrying ſome of the compound up with it. From thence this laſt had been in the act of dropping in a viſcid ſtate of fuſion, as evidently appeared when the ſpecimen was entire; having a ſtalactite and ſtalagmite correſponding accurately to each other. Unluckily I broke off the ſtalactite, but the ſtalagmite continues entire, in the form of a little cone. This new ſubſtance effervеſced in acid, but not briſkly. I watched its entire ſolution; a ſet of light clouds remained undiſſolved, and probably ſome jelly was formed; for I obſerved, that a ſeries of air-bubbles remained in the form of the fragment, and moved together without any viſible connection; thus ſeeming to indicate a chemical union between the filex and the carbonate. The fhell, fufed in the experiment, diffolved entirely in the acid, with violent effervefcence.

In the three laft experiments, and in feveral others made at the fame time, the carbonate had not been weighed; but no water being introduced to affift the compreffion, it is probable there was much lofs by internal calcination; and owing doubtlefs to this, the carbonates have crumbled almoft entirely to duft, while the compounds which they had formed with filex remain entire.

On the 13th of March, I made a fimilar experiment, in which, befides fome pounded oyfter-fhell, I introduced a mixture of chalk, with 10 *per cent.* of filex intermixed, and ground together in a mortar with water, in a ftate of cream, and then well dried. The contents of the tube when opened, were difcharged with fuch violence, that the tube was broken to pieces; but I found a lump of chalk, then in a ftate of white marble, welded to the compound; which laft, in its fracture, fhewed that irregular black colour, interfperfed roughly through a cryftalline mafs, that belongs to the alpine marbles, particularly to the kind called at Rome *Cipolline.* It was very hard and firm; I think unufually fo. It effervefced conftantly to the laft atom, in diluted nitric acid, but much more fluggifhly than the marble made of pure chalk. A cloudinefs appeared pervading all the liquid. When the effervefcence was over, a feries of bubbles continued during the whole day in the acid, without any difpofition to burft, or rife to the furface. After ftanding all next day and night, they maintained their ftation; and the folution being ftirred, was found to be entirely agglutinated into a tranfparent jelly, breaking with fharp angles. This experiment affords a direct and pofitive proof of a chemical union having taken place between the carbonate and filex.

VI.

VI.

Experiments made in Platina,—with Spar,—with Shells,—and with Carbonate of Lime of undoubted purity.

Since I had the honour of laying before this Society a ſhort ſketch of the foregoing experiments, on the 30th of Auguſt laſt (1804), many chemiſts and mineralogiſts of eminence have favoured me with ſome obſervations on the ſubject, and have ſuggeſted doubts which I am anxious to remove. It has been ſuggeſted, that the fuſibility of the carbonates may have been the conſequence of a mixture of other ſubſtances, either originally exiſting in the natural carbonate, or added to it by the contact of the porcelain tube.

With regard to the firſt of theſe ſurmiſes, I beg leave to obſerve, that, granting this cauſe of fuſion to have been the real one, a material point, perhaps all that is ſtrictly neceſſary in order to maintain this part of the Huttonian Theory, was nevertheleſs gained. For, granting that our carbonates were impure, and that their impurity rendered them fuſible, ſtill the ſame is true of almoſt every natural carbonate; ſo that our experimenats were, in that reſpect, conformable to nature. And as to the other ſurmiſe, it has been ſhewn, by comparing together a varied ſeries of experiments, that the mutual action between the lime and the porcelain was occaſioned entirely by the preſence of the carbonic acid, ſince, when it was abſent, no action of this kind took place. The fuſion of our carbonates cannot, therefore, be aſcribed to the porcelain.

Being convinced, however, by many obſervations, that the fuſibility of the carbonate did not depend upon impurity, I have exerted myſelf to remove, by freſh experiments, every doubt that has ariſen on the ſubject. In order to guard againſt natural impurities, I have applied to ſuch of my friends as have turned their attention to chemical analyſis, (a branch of the ſcience to which I have never attended,) to furniſh me with carbonate of lime of undoubted purity. To obviate the contamination ariſing from the contact of the porcelain tubes, I determined to confine the ſubject of experiment in ſome ſubſtance which had no diſpoſition to unite with the carbonate. I firſt tried charcoal, but found it very troubleſome, owing to its irregular abſorption of water and air.

I THEN turned my thoughts to the conſtruction of tubes or cups of platina for that purpoſe. Being unable readily to procure proper ſolid veſſels of this ſubſtance, I made uſe of thin laminated plates, formed into cups. My firſt method was, to fold the plate exactly as we do blotting-paper to form a filter (Fig. 26.); this produced a cup capable of holding the thinneſt liquid; and being covered with a lid, formed of a ſimilar thin plate, bent at the edges, ſo as to overlap conſiderably (Fig. 28.), the carbonate it contained was ſecured on all ſides from the contact of the porcelain tube within which it was placed. Another convenient device likewiſe occurred: I wrapt a piece of the plate of platina round a cylinder, ſo as to form a tube, each end of which was cloſed by a cover like that juſt deſcribed (Fig. 27. and 29). (In figure 26. and 27. theſe cups are repreſented upon a large ſcale, and in 28. and 29. nearly of their actual ſize). This laſt conſtruction had the advantage of containing eight or nine grains of carbonate, whereas the other would only hold about a grain and a half. On the other hand, it was not fit to retain a thin liquid; but, in moſt caſes, that circumſtance was of no conſequence; and I foreſaw that the carbonates could not

thus

thus eſcape without proving the main point under conſideration, namely, their fuſion.

The reſt of the apparatus was arranged in all reſpects as formerly deſcribed, the ſame precautions being taken to defend the platina veſſel as had been uſed with the inner tubes of porcelain.

In this manner I have made a number of experiments during this ſpring and ſummer, the reſult of which is highly ſatisfactory. They prove, in the firſt place, the propriety of the obſervations which led to this trial, by ſhewing, that the pure carbonate, thus defended from any contamination, is decidedly more refractory than chalk; ſince, in many experiments, the chalk has been reduced to a ſtate of marble, while the pure carbonate, confined in the platina veſſel, has been but very feebly acted upon, having only acquired the induration of a ſandſtone.

In other experiments, however, I have been more ſucceſsful, having obtained ſome reſults, worthy, I think, of the attention of this Society, and which I ſhall now ſubmit to their inſpection. The ſpecimens are all incloſed, for ſafety, in glaſs tubes, and ſupported on little ſtands of wax, (fig. 31, 32, 33.). The ſpecimens have, in general, been removed from the cup or tube of platina in which they were formed, theſe devices having the advantage of ſecuring both the veſſel and its contents, by enabling us to unwrap the folds without violence; whereas, in a ſolid cup or tube, it would have been difficult, after the experiment, to avoid the deſtruction either of the veſſel or its contents, or both.

April 16. 1805.—An experiment was made with pure calcareous ſpar from St Gothard, remarkably tranſparent, and having a ſtrong double refraction. A temperature of 40° was applied; but owing to ſome accident, the weight was not known. The conical cup came out clean and entire, filled

not

not quite to the brim with a yellowiſh-grey ſubſtance, having a ſhining ſurface, with longitudinal ſtreaks, as we ſometimes ſee on glaſs. This ſurface was here and there interrupted by little white tufts or protuberances, diſpoſed irregularly. On the ledge of the cup, formed by the ends of the folded platina, were ſeveral globular drops like minute pearls, viſible to the naked eye, the number of which amounted to ſixteen. Theſe ſeem to have been formed by the entire fuſion of what carbonate happened to lie on the ledge, or had been entangled amongſt the extremities of the folds, drawing itſelf together, and uniting in drops; as we ſee when any ſubſtance melts under the blowpipe. This reſult is preſerved entire, without deranging the tube. I am ſorry to find that it has begun to fall to decay, in conſequence, no doubt, of too great a loſs of its carbonic acid. But the globules do not ſeem as yet to have ſuffered any injury.

April 25.—The ſame ſpar was uſed, with two grains of water, and a heat of 33°. I have reaſon to ſuſpect, however, that, in this and ſeveral other experiments made at this time, the metal into which the cradle was plunged, on firſt introduction into the barrel, had been too hot, ſo as to drive off the water. There was a loſs of 6.4 *per cent*. The reſult lay in the cup without any appearance of frothing or ſwelling. The ſurface was of a clean white, but rough, having in one corner a ſpace ſhining like glaſs. The cup being unwrapt, the ſubſtance was obtained ſound and entire: where it had moulded itſelf on the platina, it had a ſmall degree of luſtre, with the irregular ſemitranſparency of ſaline marble: when broken, it preſerved that character more completely than in any reſult hitherto obtained; the fracture being very irregular and angular, and ſhining with facettes in various directions. I much regret that this beautiful ſpecimen

no

no longer exifts, having crumbled entirely to pieces, notwithftanding all the care I took to inclofe it with glafs and wax.

April 26.—An experiment was made with fome carbonate of lime, purified by my friend Sir George Mackenzie. Two grains of water were introduced, but were loft, I fufpect, as in the laft cafe. The heat applied was 32°. The lofs of weight was 10.6 *per cent.* Yet, though made but one day after the laft-mentioned fpecimen, it remains as frefh and entire as at firft, and promifes to continue unchanged. The external furface, as feen on removing the lid of the conical cup, was found to fhine all over like glafs, except round the edges, which were fringed with a feries of white and rough fphericles, one fet of which advanced, at one fpot, near to the centre. The fhining furface was compofed of planes, which formed obtufe angles together, and had their furface ftriated; the ftriæ bearing every appearance of a cryftalline arrangement. When freed from the cup, as before, the fubftance moulded on the platina was found to have affumed a fine pearly furface. Some large air-bubbles appeared, which had adhered to the cup, and were laid open by its removal, whofe internal furface had a beautiful luftre, and was full of ftriæ like the outward furface. The mafs is remarkable for femitranfparency, as feen particularly where the air-bubbles diminifh its thicknefs: a fmall part of the mafs being broken at one end, fhews an internal faline ftructure.

April 29.—A cup of platina was filled with feveral large pieces of a periwinkle* fhell, the fharp point of the fpiral being made to ftand upright in the cup, (fig. 30.). A heat of 30° was applied, and no water was introduced. The carbonate loft no lefs than 16 *per cent.* The fhell, particularly the

* Turbo terebra, Lin.

the ſharp end of the periwinkle, retained its original ſhape in a great meaſure, ſo as to be quite diſcernible; but the whole was glazed over with a truly vitreous luſtre. This glaze covered, at one place, a fragment of the ſhell which had been originally looſe, and had welded the two together. All the angles are rounded by this vitrifaction; the ſpace between the entire ſhell and the fragment being filled, and the angles of their meeting rounded, with this ſhining ſubſtance. The colour is a pale blue, contraſted, in the ſame little glaſs, with a natural piece of periwinkle, which is of a reddiſh-yellow. One of the fragments had adhered to the lid, and had been converted into a complete drop, of the ſize of a muſtard-ſeed. It is fixed on the wax (at *b*), along with the other ſpecimens of the experiment (fig. 32.). This reſult ſhews, as yet, no ſign of decay, notwithſtanding ſo great a loſs of weight.

The laſt experiment was repeated on the ſame day, and prepared in the ſame manner, with large fragments of ſhell, and the point of the periwinkle ſtanding up in the cup. A heat of 34° was applied; a loſs took place of 13° *per cent*. All the original form had diſappeared, the carbonate lying in the cup as a complete liquid, with a concave ſurface, which did not ſhine, but was ſtudded all over with the white ſphericles or tufts, like thoſe ſeen in the former reſults, without any ſpace between them. When detached from the cup, the ſurface moulded on the platina, was white and pearly, with a ſlight gloſs. The maſs was quite ſolid; no veſtige whatever appearing, of the original form of the fragments, (fig. 33.). A ſmall piece, broken off near the apex of the cone, ſhewed the internal ſtructure to be quite ſaline. In the act of arranging the ſpecimen on its ſtand, another piece came off in a new direction, which preſented to view the moſt perfect cryſtalline arrangement: the ſhining plane extended acroſs the whole ſpecimen, and was more than the tenth of an inch in all directions. This fracture, likewiſe,

likewiſe, ſhewed the entire internal ſolidity of the maſs. Unfortunately, this ſpecimen has ſuffered much by the ſame decay to which all of them are ſubject which have loſt any conſiderable weight. The part next the outward ſurface alone remains entire. I have never been able to explain, in a ſatisfactory manner, this difference of durability; the laſt-mentioned reſult having loſt more in proportion to its weight than this.

About the beginning of June, I received from Mr Hatchett ſome pure carbonate of lime, which he was ſo good as to prepare, with a view to my experiments; and I have been conſtantly employed with it till within theſe few days.

My firſt experiments with this ſubſtance were peculiarly unfortunate, and it ſeemed to be leſs eaſily acted upon than any ſubſtance of the kind I had tried. Its extreme purity, no doubt, contributed much to this, though another circumſtance had likewiſe had ſome effect. The powder, owing to a cryſtallization which had taken place on its precipitation, was very coarſe, and little ſuſceptible of cloſe ramming; the particles, therefore, had leſs advantage than when a fine powder is uſed, in acting upon each other, and I did not chooſe to run any riſk of contamination, by reducing the ſubſtance to a finer powder. Whatever be the cauſe, it is certain, that in many experiments in which the chalk was changed to marble, this ſubſtance remained in a looſe and brittle ſtate, though conſiſting generally of clear and ſhining particles. I at laſt, however, ſucceeded in obtaining ſome very good reſults with this carbonate.

In an experiment made with it on the 18th of June, in a ſtrong heat, I obtained a very firm maſs with a ſaline fracture, moulded in ſeveral places on the platina, which was now uſed in the cylindrical form. On the 23d, in a ſimilar experiment, the barrel failed, and the ſubject of experiment was found in an entire ſtate of froth, proving its former fluidity.

On the 25th, in a fimilar experiment, a heat of 64° was applied without any water within the barrel. The platina tube, (having been contaminated in a former experiment with fome fufible metal), melted, and the carbonate retaining its cylindrical fhape, had fallen through it, fo as to touch the piece of porcelain which had been placed next to the platina tube. At the point of contact, the two had run together, as a hot iron runs when touched by fulphur. The carbonate itfelf was very tranfparent, refembling a piece of fnow in the act of melting.

On the 26th of June, I made an experiment with this carbonate, which afforded a beautiful refult. One grain of water was introduced with great care; yet there was a lofs of 6.5 *per cent.*, and the refult has fallen to decay. The pyrometer indicated 43°. On the outfide of the platina cylinder, and on one of the lids, were feen a fet of globules, like pearls, as once before obtained, denoting perfect fufion. When the upper lid was removed, the fubftance was found to have funk almoft out of fight, and had affumed a form not eafily defcribed. (I have endeavoured to reprefent it in fig. 31. by an ideal fection of the platina-tube and its contents, made through the axis of the cylinder). The powder, firft fhrinking upon itfelf in the act of agglutination, had formed a cylindrical rod, a remnant of which (*a b c*) ftood up in the middle of the tube. By the continued action of heat, the fummit of the rod (at *a*) had been rounded in fufion, and the mafs being now foftened, had funk by its weight, and fpread below, fo as to mould itfelf in the tube, and fill its lower part completely (*d f g e*). At the fame time, the vifcid fluid adhering to the fides (at *e* and *d*), while the middle part was finking, had been in part left behind, and in part drawn out into a thin but tapering fhape, united by a curved furface (at *b* and *c*) to the middle rod. When the platina tube was unwrapt, the thin edges (at *e* and *d*) were preferved all round, and in a

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ſtate of beautiful ſemitranſparency. (I have attempted to repreſent the entire ſpecimen, as it ſtood on its cone of wax, in fig. 34.). The carbonate, where moulded on the platina, had a clean pearly whiteneſs, with a ſaline appearance externally, and in the ſun, ſhone with facettes. Its ſurface was interruptted by a few ſcattered air-bubbles, which had lain againſt the tube. The intervening ſubſtance was unuſually compact and hard under the knife. The whole ſurface (*e b a c d*, fig. 31.), and the inſide of the air-bubbles, had a vitreous luſtre. Thus, every thing denoted a ſtate of viſcid fluidity, like that of honey.

These laſt experiments ſeem to obviate every doubt that remained with reſpect to the fuſibility of the pureſt carbonate, without the aſſiſtance of any foreign ſubſtance.

VII.

Meaſurement of the Force required to conſtrain the Carbonic Acid.—Apparatus with the Muzzle of the Barrel upwards, and the weight acting by a long Lever.—Apparatus with the Muzzle downwards.—Apparatus with Weight acting directly on the barrel.—Compariſon of various reſults.

In order to determine, within certain limits at leaſt, what force had been exerted in the foregoing experiments, and what was neceſſary to enſure their ſucceſs, I made a number of experiments, in a mode nearly allied to that followed by Count Rumford, in meaſuring the exploſive force of gunpowder.

I began to uſe the following ſimple apparatus in June 1803. I took one of the barrels, made as above deſcribed, for the purpoſe of compreſſion, having a bore of 0.75 of an

 inch,

inch *, and dreſſed its muzzle to a ſharp edge. To this barrel was firmly ſcrewed a collar of iron (*a a*, fig. 36.) placed at a diſtance of about three inches from the muzzle, having two ſtrong bars (*b b*) projecting at right angles to the barrel, and dreſſed ſquare. The barrel, thus prepared, was introduced, with its breech downwards, into the vertical muffle (fig. 35.); its length being ſo adjuſted, that its breech ſhould be placed in the ſtrongeſt heat; the two projecting bars above deſcribed, reſting on two other bars (*c c*, fig. 35.) laid upon the furnace to receive them; one upon each ſide of the muffle. Into the barrel, ſo placed, was introduced a cradle, containing carbonate, with all the arrangements formerly mentioned; the rod connected with it being of ſuch length, as juſt to lie within the muzzle of the barrel. The liquid metal was then poured in till it filled the barrel, and ſtood at the muzzle with a convex ſurface; a cylinder of iron, of about an inch in diameter, and half an inch thick, was laid on the muzzle (fig. 35. and 37.), and to it a compreſſing weight was inſtantly applied. This was firſt done by the preſſure of a bar of iron (*d e*, fig. 35.), three feet in length, introduced looſely into a hole (*d*), made for the purpoſe in the wall againſt which the furnace ſtood; the diſtance between this hole and the barrel being one foot. A weight was then ſuſpended at the extremity of the bar (*e*), and thus a compreſſing force was applied, equal to three times that weight. In the courſe of practice, a cylinder of lead was ſubſtituted for that of iron, and a piece of leather was placed between it and the muzzle of the barrel, which laſt being dreſſed to a pretty ſharp edge, made an impreſſion in the lead: to aſſiſt this effect, one ſmart blow of a hammer was ſtruck upon the bar, directly over the barrel, as ſoon as the weight had been hung on.

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* This was the ſize of barrel uſed in all the following experiments, where the fact is not otherwiſe expreſſed.

It was eſſential, in this mode of operation, that the whole of the metal ſhould continue in a liquid ſtate during the action of heat; but when I was ſatisfied as to its intenſity and duration, I congealed the metal, either by extinguiſhing the furnace entirely, or by pouring water on the barrel. As ſoon as the heat began to act, drops of metal were ſeen to force themſelves between the barrel and the leather, following each other with more or leſs rapidity, according to circumſtances. In ſome experiments, there was little exudation; but few of them were entirely free from it. To ſave the metal thus extruded, I placed a black-lead crucible, having its bottom perforated, round the barrel, and luted cloſe to it, (fig. 37.); ſome ſand being laid in this crucible, the metal was collected on its ſurface. On ſome occaſions, a ſound of ebullition was heard during the action of heat; but this was a certain ſign of failure.

The reſults of the moſt important of theſe experiments, have been reduced to a common ſtandard in the ſecond table placed in the Appendix; to which reference is made by the following numbers.

No. 1.—On the 16th of June 1803, I made an experiment with theſe arrangements. I had tried to uſe a weight of 30 lb. producing a preſſure of 90 lb., but I found this not ſufficient. I then hung on a weight of 1 cwt., or 112 lb.; by which a compreſſing force was applied of 3 cwt. or 336 lb. Very little metal was ſeen to eſcape, and no ſound of ebullition was heard. The chalk in the body of the large tube was reduced to quicklime; but what lay in the inner tube was pretty firm, and effervеſced to the laſt. One or two facettes, of good appearance, were likewiſe found. The contents of the ſmall tube had loſt but 2.6 *per cent.*; but there was a ſmall viſible intruſion of metal, and the reſult, by its appearance, indicated a greater loſs. I conſidered this, however, as one point gained; that being the

firſt

firſt tolerable compreſſion accompliſhed by a determinate force. The pyrometer indicated 22°.

This experiment was repeated the ſame day, when a ſtill ſmaller quantity of metal eſcaped at the muzzle; but the barrel had given way below, in the manner of thoſe that have yielded for want of ſufficient air. Even this reſult was ſatisfactory, by ſhewing that a mechanical power, capable of forcing ſome of the barrels, could now be commanded. The carbonate in the little tube had loſt 20 *per cent.;* but part of it was in a hard and firm ſtate, efferveſcing to the laſt.

No. 2.—On the 21ſt June, I made an experiment with another barrel, with the ſame circumſtances. I had left an empty ſpace in the large tube, and had intended to introduce its muzzle downwards, meaning that ſpace to anſwer as an air-tube; but it was inverted by miſtake, and the tube entering with its muzzle upwards, the empty ſpace had of courſe filled with metal, and thus the experiment was made without any included air. There was no pyrometer uſed; but the heat was gueſſed to be about 25° where the ſubject of experiment lay. The barrel, when opened, was found full of metal, and the cradle being laid flat on the table, a conſiderable quantity of metal ran from it, which had undoubtedly been lodged in the vacuity of the large tube. When cold, I found that vacuity ſtill empty, with a plating of metal. The tube was very clean to appearance, and, when ſhaken, its contents were heard to rattle. Above the little tube, and the cylinder of chalk, I had put ſome borax and ſand, with a little pure borax in the middle, and chalk over it. The metal had not penetrated beyond the borax and ſand, by a good fortune peculiar to this experiment; the intruſion of metal in this mode of execution, being extremely troubleſome. The button of chalk, was found in a ſtate of clean white carbonate, and pretty hard, but without tranſparency. The little

tube

tube was perfectly clean. Its weight with its contents, seemed to have suffered no change from what it had been when first introduced. Attending, however, to the balance with scrupulous nicety, a small preponderance did appear on the side of the weight. This was done away by the addition of the hundredth of a grain to the scale in which the carbonate lay, and an addition of another hundredth produced in it a decided preponderance. Perhaps, had the tube, before its introduction, been examined with the same care, as great a difference might have been detected; and it seems as if there had been no loss, at least not more than one hundredth of a grain, which on 10.95 grains, amounts to 0.0912, say 0.1 *per cent.* The carbonate was loose in the little tube, and fell out by shaking. It had a yellow colour, and compact appearance, with a stony hardness under the knife, and a stony fracture; but with very slight facettes, and little or no transparency. In some parts of the specimen, a whitish colour seemed to indicate partial calcination. On examining the fracture, I perceived, with the magnifier, a small globule of metal, not visible to the naked eye, quite insulated and single. Possibly the substance may have contained others of the same sort, which may have compensated for a small loss, but there could not be many such, from the general clean appearance of the whole. In the fracture, I saw here and there small round holes, seeming, though imperfectly, to indicate a beginning of ebullition.

I MADE a number of experiments in the same manner, that is to say, with the muzzle of the barrel upwards, in some of which I obtained very satisfactory results; but it was only by chance that the substance escaped the contamination of the fusible metal; which induced me to think of another mode of applying the compressing weight with the muzzle of the barrel downwards, by which I expected to repeat, with a determinate weight, all the experiments formerly made

made in barrels clofed by congealed metal; and that, by making ufe of an air-tube, the air, rifing to the breech, would fecure the contents of the tube from any contamination. In this view, the barrel was introduced from below into the muffle with its breech upwards, and retained in that pofition by means of a hook fixed to the furnace, till the collar was made to prefs up againft the grate, by an iron lever, loaded with a weight, and refting on a fupport placed in front. In fome experiments made in this way, the refult was obtained very clean, as had been expected; but the force had been too feeble, and when it was increafed, the furnace yielded upwards by the mechanical ftrain.

I FOUND it therefore neceffary to ufe a frame of iron, (as in fig. 38.; the frame being reprefented feparately in fig. 39.), by which the brick-work was relieved from the mechanical ftrain. This frame confifted of two bars *(a b* and *f e*, figs. 38. and 39.), fixed into the wall, (at *a* and *f*,) paffing horizontally under the furnace, one on each fide of the muffle, turning downwards at the front, (in *b* and *e*), and meeting at the ground, with a flat bar *(c d)* uniting the whole. In this manner, a kind of ftirrup (*b c d e*) was formed in front of the furnace, upon the crofs bar *(c d)* of which a block of wood (*h h*, fig. 38.), was placed, fupporting an edge of iron, upon which the lever refted; the working end of the lever *(g)* acting upwards. A ftrain was exerted, by means of the barrel and its collar, againft the horizontal bars, *(a b* and *f e)*, which was effectually refifted by the wall (at *a* and *f)* at one end of thefe bars, and by the upright bars (*c b* and *d e)* at the other end. In this manner the whole ftrain was fuftained by the frame, and the furnace ftood without injury.

THE iron bar, at its working end, was formed into the fhape of a cup, (at *g*), and half filled with lead, the fmooth furface of which, was applied to the muzzle of the barrel. The lever, too, was lengthened, by joining to the bar of iron, a beam of wood,

making

making the whole ten feet in length. In this manner, a preſſure upwards was applied to the barrel, equal to the weight of 10 cwt.

In the former method, in which the barrel ſtood with its muzzle upwards, the weight was applied while the metal was liquid. In this caſe, it was neceſſary to let it previouſly congeal, otherwiſe the contents would have run out in placing the barrel in the muffle, and to allow the liquefaction eſſential to theſe trials, to be produced by the propagation of heat from the muffle downwards. This method required, therefore, in every caſe, the uſe of an air-tube; for without it, the heat acting upon the breech, while the metal at the muzzle was ſtill cold, would infallibly have deſtroyed the barrel. A great number of theſe experiments failed, with very conſiderable waſte of the fuſible metal, which, on theſe occaſions was nearly all loſt. But a few of them ſucceeded, and afforded very ſatisfactory reſults, which I ſhall now mention.

In November 1803, ſome good experiments were made in this way, all with a bore of 0.75, and a preſſure of 10 cwt.

No. 3.—On the 19th, a good limeſtone was obtained in an experiment made in a temperature of 21°, with a loſs of only 1.1 *per cent.*

No. 4.—On the 22d, in a ſimilar experiment, there was little exudation by the muzzle. The pyrometer gave 31°. The carbonate was in a porous, and almoſt frothy ſtate.

No. 5.—In a ſecond experiment, made the ſame day, the heat roſe to 37° or 41°. The ſubſtance bore ſtrong marks of fuſion, the upper part having ſpread on the little tube: the whole was very much ſhrunk, and run againſt one ſide. The maſs ſparkling and white, and in a very good ſtate.

No. 6.—On the 25th, an experiment was made with chalk, and ſome fragments of ſnail ſhell, with about half a grain of water. The heat had riſen to near 51° or 49°. The barrel had been held tight by the beam, but was rent and a little ſwelled at the breech. The rent was wide, and ſuch as has always appeared in the ſtrongeſt barrels when they failed. The carbonate was quite calcined, it had boiled over the little tube, and was entirely in a frothy ſtate, with large and diſtinctly rounded air-holes. The fragments of ſhell which had occupied the upper part of the little tube, had loſt every trace of their original ſhape in the act of ebullition and fuſion.

No. 7.—On the 26th a ſimilar experiment was made, in which the barrel was thrown open, in ſpite of this powerful compreſſing force, with a report like that of a gun, (as I was told, not having been preſent), and the bar was found in a ſtate of ſtrong vibration. The carbonate was calcined, and ſomewhat frothy, the heart of one piece of chalk uſed was in a ſtate of ſaline marble.

It now occurred to me to work with a compreſſing force, and no air-tube, truſting, as happened accidentally in one caſe, that the expanſion of the liquid would clear itſelf by gentle exudation, without injury to the carbonate. In this mode, it was neceſſary, for reaſons lately ſtated, to place the muzzle upwards. Various trials made thus, at this time, afforded no remarkable reſults. But I reſumed the method, with the following alteration in the application of the weight, on the 27th of April 1804.

I conceived that ſome inconvenience might ariſe from the mode of employing the weight in the former experiments. In them it had been applied at the end of the bar, and its effect propagated along it, ſo as to preſs againſt the barrel at its other extremity. It occurred to me, that the propagation of motion in this way, requiring ſome ſenſible time, a conſiderable quantity of carbonic acid might eſcape by a ſudden eruption, before that propagation had taken effect. I therefore thought, that more effectual work might be done, by placing

placing a heavy mafs, (fig. 40.), fo as to act directly and fimply upon the muzzle of the barrel; this mafs being guided and commanded by means of a powerful lever, *(a b)*. For this purpofe, I procured an iron roller, weighing 3 cwt. 7 lb., and fufpended it over the furnace, to the end of a beam of wood, refting on a fupport near the furnace, with a long arm guided by a rope *(c c)* and pulley *(d)*, by which the weight could be raifed or let down at pleafure.

WITH this apparatus I made fome tolerable experiments; but I found the weight too light to afford certain and fteady refults of the beft quality. I therefore procured at the foundry a large mafs of iron *(f)*, intended, I believe, for driving piles, and which, after allowing for the counterpoife of the beam, gave a direct preffure of 8.1 cwt.; and I could, at pleafure, diminifh the compreffing force, by placing a bucket (*e*) at the extremity of the lever, into which I introduced weights, whofe effect on the ultimate great mafs, was known by trial. Many barrels failed in thefe trials: at laft, I obtained one of fmall bore, inch 0.54, which gave two good refults on the 22d of June 1804.

No. 8.—WISHING to afcertain the leaft compreffing force by which the carbonate could be effectually conftrained in melting heats, I firft obferved every thing ftanding firm in a heat of above 20°; I then gradually threw weights into the bucket, till the compreffing force was reduced to 2 cwt. Till then, things continued fteady; but, on the preffure being ftill further diminifhed, metal began to ooze out at the muzzle, with increafing rapidity. When the preffure was reduced to $1\frac{1}{2}$ cwt. air rufhed out with a hiffing noife. I then ftopped the experiment, by pouring water on the barrel. The piece of chalk had loft 12 *per cent.* It was white and foft on the outfide, but firm and good in the heart.

No. 9.—An experiment was made with chalk, in a little tube; to this, one grain of water was added. I had intended to work with 4 cwt. only; but the barrel was no fooner placed, than an exudation of metal began at the muzzle, owing, doubtlefs, to the elafticity of the water. I immediately increafed the preffure to 8.1 cwt. by removing the weight from the bucket, when the exudation inftantly ceafed. I continued the fire for three quarters of an hour, during which time no exudation happened; then all came out remarkably clean, with fcarcely any contamination of metal. The lofs amounted to 2.58 *per cent.* The fubftance was tolerably indurated, but had not acquired the character of a complete ftone.

In thefe two laft experiments, the bore being fmall, a pyrometer could not be admitted.

On the 5th of July 1804, I made three very fatisfactory experiments of this kind, in a barrel with the large bore of 0.75 of an inch.

No. 10.—was made with a compreffing force of only 3 cwt. A fmall eruption at the muzzle being obferved, water was thrown on the barrel: the pyrometer gave 21°: the chalk was in a firm ftate of limeftone.

No. 11.—with 4 cwt. The barrel ftood without any eruption or exudation, till the heat rofe to 25°. There was a lofs of 3.6 *per cent.*: the refult was fuperior, in hardnefs and tranfparency, to the laft, having fomewhat of a faline fracture.

No. 12.—with 5 cwt. The refult, with a lofs of 2.4 *per cent.*, was of a quality fuperior to any of thofe lately obtained.

These experiments appear to anfwer the end propofed, of afcertaining the leaft preffure, and loweft heat, in which limeftone can be formed. The refults, with various barrels of different fizes, agree tolerably, and tend to confirm each other. The table fhews, when we compare numbers 1, 2, 8, 10, 11, 12, That a preffure of 52 atmofpheres, or 1700 feet of fea, is capable

capable of forming a limeſtone in a proper heat: That under 86 atmoſpheres, anſwering nearly to 3000 feet, or about half a mile, a complete marble may be formed: and laſtly, That with a preſſure of 173 atmoſpheres, or 5700 feet, that is, little more than one mile of ſea, the carbonate of lime is made to undergo complete fuſion, and to act powerfully on other earths.

VIII.

Formation of Coal.—Accidental occurrence which led me to undertake theſe Experiments.—Reſults extracted from a former publication.—Explanation of ſome difficulties that have been ſuggeſted.—The Fibres of Wood in ſome caſes obliterated, and in ſome preſerved under compreſſion.—Reſemblance which theſe Reſults bear to a ſeries of Natural Subſtances deſcribed by Mr Hatchett.*—Theſe reſults ſeem to throw light on the hiſtory of Surturbrand.*

As I intend, on ſome future occaſion, to reſume my experiments with inflammable ſubſtances, which I look upon as far from complete, I ſhall add but a few obſervations to what I have already laid before this Society, in the ſketch I had the honour to read in this place on the 30th of Auguſt laſt.

The following incidental occurrence led me to enter upon this ſubject rather prematurely, ſince I had determined firſt to ſatisfy myſelf with regard to the carbonate of lime.

Observing, in many of the laſt-mentioned claſs of experiments, that the elaſtic matters made their eſcape between the muzzle of the barrel and the cylinder of lead, I was in the habit, as mentioned above, of placing a piece of leather between the lead and the barrel; in which poſition, the heat to which the leather was expoſed, was neceſſarily below that of melting lead.

lead. In an experiment, made on the 28th November 1803, in order to afcertain the power of the machinery, and the quantity of metal driven out by the expanfion of the liquid, there being nothing in the barrel but metal, I obferved, as foon as the comprefling apparatus was removed, (which on this occafion was done while the lower part of the barrel was at its full heat, and the barrel ftanding brim full of liquid metal,) that all the leather which lay on the outfide of the circular muzzle of the barrel, remained, being only a little browned and crumpled by the heat to which it had been expofed. What leather lay within the circle, had difappeared; and, on the furface of the liquid metal, which ftood up to the lip of the barrel, I faw large drops, of a fhining black liquid, which, on cooling, fixed into a crifp black fubftance, with a fhining fracture, exactly like pitch or pure coal. It burned, though not with flame. While hot, it fmelt decidedly of volatile alkali. The important circumftance here, is the different manner in which the heat has acted on the leather, without and within the rim of the barrel. The only difference confifted in compreffion, to which, therefore, the difference of effect muft be afcribed: by its force, the volatile matter of the leather which efcaped from the outward parts, had within the rim, been conftrained to remain united to the reft of the compofition, upon which it had acted as a flux, and the whole together had entered into a liquid ftate, in a very low heat. Had the preffure been continued till all was cool, thefe fubftances muft have been retained, producing a real coal.

On the 24th April 1803, a piece of leather ufed in a fimilar manner, (the compreffing force being continued, however, till all was cold,) was changed to a fubftance like glue, owing doubtlefs to compreffion, in a heat under that of melting lead.

These obfervations led me to make a feries of experiments with animal and vegetable fubftances, and with coal; the

the refult of which I have already laid before the Society. I fhall now repeat that communication, as printed in NICHOLSON's *Journal* for October laft (1804).

" I HAVE likewife made fome experiments with coal, treated in the fame manner as the carbonate of lime: but I have found it much lefs tractable; for the bitumen, when heat is applied to it, tends to efcape by its fimple elafticity, whereas the carbonic acid in marble, is in part retained by the chemical force of quicklime. I fucceeded, however, in conftraining the bituminous matter of the coal, to a certain degree, in red heats, fo as to bring the fubftance into a complete fufion, and to retain its faculty of burning with flame. But, I could not accomplifh this in heats capable of agglutinating the carbonate; for I have found, where I rammed them fucceffively into the fame tube, and where the veffel has withftood the expanfive force, that the carbonate has been agglutinated into a good limeftone, but that the coal has loft about half its weight, together with its power of giving flame when burnt, remaining in a very compact ftate, with a fhining fracture. Although this experiment has not afforded the defired refult, it anfwers another purpofe admirably well. It is known, that where a bed of coal is croffed by a dike of whinftone, the coal is found in a peculiar ftate in the immediate neighbourhood of the whin: the fubftance in fuch places being incapable of giving flame, it is diftinguifhed by the name of *blind coal.* Dr HUTTON has explained this fact, by fuppofing that the bituminous matter of the coal, has been driven by the local heat of whin, into places of lefs intenfity, where it would probably be retained by diftillation. Yet the whole muft have been carried on under the action of a preffure capable of conftraining the carbonic acid of the calcareous fpar, which occurs frequently in fuch rocks. In the laft-mentioned experiment, we have a perfect reprefentation

tation of the natural fact; ſince the coal has loſt its petroleum, while the chalk in contact with it has retained its carbonic acid.

" I HAVE made ſome experiments of the ſame kind, with vegetable and animal ſubſtances. I found their volatility much greater than that of coal, and I was compelled, with them, to work in heats below redneſs; for, even in the loweſt red-heat, they were apt to deſtroy the apparatus. The animal ſubſtance I commonly uſed was horn, and the vegetable, ſaw-duſt of fir. The horn was incomparably the moſt fuſible and volatile of the two. In a very ſlight heat, it was converted into a yellow red ſubſtance, like oil, which penetrated the clay tubes through and through. In theſe experiments, I therefore made uſe of tubes of glaſs. It was only after a conſiderable portion of the ſubſtance had been ſeparated from the maſs, that the remainder aſſumed the clear black peculiar to coal. In this way I obtained coal, both from ſaw-duſt and from horn, which yielded a bright flame in burning.

" THE mixture of the two produced a ſubſtance having exactly the ſmell of ſoot or coal-tar. I am therefore ſtrongly inclined to believe, that animal ſubſtance, as well as vegetable, has contributed towards the formation of our bituminous ſtrata. This ſeems to confirm an opinion, advanced by Mr KEIR, which has been mentioned to me ſince I made this experiment. I conceive, that the coal which now remains in the world, is but a ſmall portion of the organic matter originally depoſited: the moſt volatile parts have been driven off by the action of heat, before the temperature had riſen high enough to bring the ſurrounding ſubſtance into fuſion, ſo as to confine the elaſtic fluids, and ſubject them to compreſſion.

" IN ſeveral of theſe experiments, I found that, when the preſſure was not great, when equal, for inſtance, only to 80 atmoſpheres, that the horn employed was diſſipated entirely, the glaſs glafs tube which had contained it being left almoft clean: yet undoubtedly, if expofed to heat without compreffion, and protected from the contact of the atmofphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility. Here, then, it would feem as if the moderate preffure, by keeping the elements of the fubftance together, had promoted the general volatility, without being ftrong enough to refift that expanfive force, and thus, that the whole had efcaped. This refult, which I fhould certainly not have forefeen in theory, may perhaps account for the abfence of coal in fituations where its prefence might be expected on principles of general analogy."

SINCE this publication, a very natural queftion has been put to me. When the inflammable fubftance has loft weight, or when the whole has been diffipated, in thefe experiments, what has become of the matter thus driven off?

I MUST own, that to anfwer this queftion with perfect confidence, more experiments are required. But, in the courfe of practice, two circumftances have occurred as likely, in moft cafes, to have occafioned the lofs alluded to. I found in thefe experiments, particularly with horn, that the chalk, both in powder and in lump, which was ufed to fill vacuities in the tubes, and to fix them in the cradle, was ftrongly impregnated with an oily or bituminous matter, giving to the fubftance the qualities of a ftinkftone. I conceive, that the moft volatile part of the horn has been conveyed to the chalk, partly in a ftate of vapour, and partly by boiling over the lips of the glafs tube; the whole having been evidently in a ftate of very thin fluidity. Having, in fome cafes, found the tube, which had been introduced full of horn, entirely empty after the experiment, I was induced, as above ftated, to conceive, that, under preffure, it had acquired a greater general volatility than it had in freedom; and I find that, in the open fire, horn yields a charcoal equal to 20 *per cent.* of the original weight. But more experiments muſt be made on this ſubject.

ANOTHER cauſe of the loſs of weight, lay undoubtedly in the exceſs of heat employed in moſt of them, to remove the cradle from the barrel. With inflammable ſubſtances, no air-tube was uſed, and the heats being low, the air lodged in interſtices had been ſufficient to ſecure the barrels from deſtruction, by the expanſion of the liquid metal. In this view, likewiſe, I often uſed lead, whoſe expanſion in ſuch low heats, I expected to be leſs than that of the fuſible metal. And the lead requiring to melt it, a heat very near to that of redneſs, the ſubject of experiment was thus, on removing the cradle, expoſed in freedom to a temperature which was comparatively high. But, obſerving that a great loſs was thus occaſioned, I returned to the uſe of the fuſible metal, together with my former method of melting it, by plunging the barrel, when removed from the furnace, into a ſolution of muriate of lime, by which it could only receive a heat of 250° of FAHRENHEIT.

THE effect was remarkable, in the few experiments tried in this way. The horn did not, as in the other experiments, change to a hard black ſubſtance, but acquired a ſemifluid and viſcid conſiſtency, with a yellow-red colour, and a very offenſive ſmell. This ſhews, that the ſubſtances which here occaſioned both the colour and ſmell of the reſults, had been driven off in the other experiments, by the too great heat applied to the ſubſtance, when free from compreſſion.

I FOUND that the organization of animal ſubſtance was entirely obliterated by a ſlight action of heat, but that a ſtronger heat was required to perform the entire fuſion of vegetable matter. This, however, was accompliſhed; and in ſeveral experiments, pieces of wood were changed to a jet-black and inflammable ſubſtance, generally very porous, in which no

trace

trace could be difcovered of the original organization. In others, the vegetable fibres were ftill vifible, and are forced afunder by large and fhining air-bubbles.

Since the publication of the fketch of my experiments, I have had the pleafure to read Mr Hatchett's very interefting account of various natural fubftances, nearly allied to coal; and I could not help being ftruck with the refemblance which my refults bear to them, through all their varieties, as brought into view by that able chemift; that refemblance affording a prefumption, that the changes which, with true fcientific modefty, he afcribes to an unknown caufe, may have refulted from various heats acting under preffure of various force. The fubftance to which he has given the name of *Retinafphaltum*, feems to agree very nearly with what I have obtained from animal fubftance, when the barrel was opened by means of low heat. And the fpecimen of wood entering into fufion, but ftill retaining the form of its fibres, feems very fimilar to the intermediate fubftance of Bovey-coal and *Surturbrand*, which Mr Hatchett has affimilated to each other. It is well known, that the furturbrand of Iceland, confifts of the ftems of large trees, flattened to thin plates, by fome operation of nature hitherto unexplained. But the laft-mentioned experiment feems to afford a plaufible folution of this puzzling phenomenon.

In all parts of the globe, we find proofs of flips, and various relative motions, having taken place amongft great maffes of rock, whilft they were foft in a certain degree, and which have left unequivocal traces behind them, both in the derangement of the beds of ftrata, and in a fmooth and fhining furface, called *flickenfide*, produced by the direct friction of one mafs on another. During the action of fubterranean heat, were a fingle ftratum to occur, containing trees intermixed with animal fubftances, fhell-fifh, &c. thefe trees would be reduced, to a foft and unctuous ftate, fimilar to that of the piece of wood in the laſt-mentioned experiment, whilſt the ſubſtance of the contiguous ſtrata retained a conſiderable degree of firmneſs. In this ſtate of things, the ſtratum juſt mentioned, would very naturally become the ſcene of a ſlip, occaſioned by the unequal preſſure of the ſurrounding maſſes. By ſuch a ſliding motion, accompanied by great compreſſion, a tree would be flattened, as any ſubſtance is ground in a mortar, by the combination of a lateral and direct force. At the ſame time, the ſhells along with the trees, would be flattened, like thoſe deſcribed by BERGMAN; while thoſe of the ſame ſpecies in the neighbouring limeſtone-rock, being protected by its inferior fuſibility, would retain their natural ſhape.

IX.

Application of the foregoing reſults to Geology.—The fire employed in the Huttonian Theory is a modification of that of the Volcanoes.—This modification muſt take place in a lava previous to its eruption.—An Internal Lava is capable of melting Limeſtone.—The effects of Volcanic Fire on ſubſtances in a ſubterranean and ſubmarine ſituation, are the ſame as thoſe aſcribed to Fire in the Huttonian Theory.—Our Strata were once in a ſimilar ſituation, and then underwent the action of fire.—All the conditions of the Huttonian Theory being thus combined, the formation of all Rocks may be accounted for in a ſatisfactory manner.—Concluſion.

HAVING inveſtigated, by means of the foregoing experiments, ſome of the chemical ſuppoſitions involved in the Huttonian Theory, and having endeavoured to aſſign a determinate limit to the power of the agents employed; I ſhall now apply theſe reſults to Geology, and inquire how far the events

ſuppoſed

ſuppoſed anciently to have taken place, accord with the exiſting ſtate of our globe.

The moſt powerful and eſſential agent of the Huttonian Theory, is Fire, which I have always looked upon as the ſame with that of volcanoes, modified by circumſtances which muſt, to a certain degree, take place in every lava previous to its eruption.

The original ſource of internal fire is involved in great obſcurity; and no ſufficient reaſon occurs to me for deciding whether it proceeds by emanation from ſome vaſt central reſervoir, or is generated by the local operation of ſome chemical proceſs. Nor is there any neceſſity for ſuch a deciſion: all we need to know is, that internal fire exiſts, which no one can doubt, who believes in the eruptions of Mount Veſuvius. To require that a man ſhould account for the generation of internal fire, before he is allowed to employ it in geology, is no leſs abſurd than it would be to prevent him from reaſoning about the conſtruction of a teleſcope, till he could explain the nature of the ſun, or account for the generation of light *. But while we remain in ſuſpenſe as to the prime cauſe of this tremendous agent, many circumſtances of importance with regard to it, may fairly become the ſubjects of obſervation and diſcuſſion.

Some authors (I conceive through ignorance of the facts) have alleged, that the fire of Ætna and Veſuvius is merely ſuperficial. But the depth of its action is ſufficiently proved, by the great diſtance to which the eruptive percuſſions are felt, and ſtill more, by the ſubſtances thrown out uninjured by ſome eruptions

* This topic, however, has of late been much urged againſt us, and an unfair advantage has been taken of what Mr Playfair has ſaid upon it. What he gave as mere conjecture on a ſubject of collateral importance, has been argued upon as the baſis and fundamental doctrine of the ſyſtem.

eruptions of Mount Vefuvius. Some of thefe, as marble and gypfum, are incapable in freedom of refifting the action of fire. We have likewife granite, fchiftus, gneifs, and ftones of every known clafs, befides many which have never, on any other occafion, been found at the furface of our globe. The circumftance of thefe fubftances having been thrown out, unaffected by the fire, proves, that it has proceeded from a fource, not only as deep, but deeper, than their native beds; and as they exhibit fpecimens of every clafs of minerals, the formation of which we pretend to explain, we need inquire no further into the depth of the Vefuvian fire, which has thus been proved to reach below the range of our fpeculations.

Volcanic fire is fubject to perpetual and irregular variations of intenfity, and to fudden and violent renewal, after long periods of abfolute ceffation. Thefe variations and intermiffions, are likewife effential attributes of fire as employed by Dr Hutton; for fome geological fcenes prove, that the indurating caufe has acted repeatedly on the fame fubftance, and that, during the intervals of that action, it had ceafed entirely. This circumftance affords a complete anfwer to an argument lately urged againft the Huttonian Theory, founded on the wafte of heat which muft have taken place, as it is alleged, through the furface. For if, after abfolute ceffation, a power of renewal exifts in nature, the idea of wafte by continuance is quite inapplicable.

The external phenomena of volcanoes are fufficiently well known; but our fubject leads us to inquire into their internal actions. This we are enabled to do by means of the foregoing experiments, in fo far as the carbonate of lime is concerned.

Some experiments which I formerly * laid before this Society and the public, combined with thofe mentioned in this paper,

* *Edinburgh Tranfactions*, Vol. V. Part I. p. 60—66.

paper, prove, that the feebleſt exertions of volcanic fire, are of ſufficient intenſity to perform the agglutination, and even the entire fuſion, of the carbonate of lime, when its carbonic acid is effectually confined by preſſure; for though lava, after its fuſion, may be made, in our experiments, to congeal into a glaſs, in a temperature of 16° or 18° of WEDGWOOD, in which temperature the carbonate would ſcarcely be affected; it muſt be obſerved, that a ſimilar congelation is not to be looked for in nature; for the maſs, even of the ſmalleſt ſtream of lava, is too great to admit of ſuch rapid cooling. And, in fact, the external part of a lava is not vitreous, but conſiſts of a ſubſtance which, as my experiments have proved, muſt have been congealed in a heat of melting ſilver, that is, in 22° of WEDGWOOD; while its internal parts bear a character indicating that they congealed in 27° or 28° of the ſame ſcale. It follows, that no part of the lava, while it remained liquid, can have been leſs hot than 22° of WEDGWOOD. Now, this happens to be a heat, in which I have accompliſhed the entire fuſion of the carbonate of lime, under preſſure. We muſt therefore conclude, that the heat of a running lava is always of ſufficient intenſity to perform the fuſion of limeſtone.

IN every active volcano, a communication muſt exiſt between the ſummit of the mountain and the unexplored region, far below its baſe, where the lava has been melted, and whence it has been propelled upwards; the liquid lava riſing through this internal channel, ſo as to fill the crater to the brim, and flow over it. On this occaſion, the ſides of the mountain muſt undergo a violent hydroſtatical preſſure outwards, to which they often yield by the formation of a vaſt rent, through which the lava is diſcharged in a lateral eruption, and flows in a continued ſtream ſometimes during months. On Ætna moſt of the eruptions are ſo performed; few lavas flowing from the ſummit, but generally breaking out laterally, at very elevated ſtations.

At

At the place of delivery, a quantity of gaſeous matter is propelled violently upwards, and, along with it, ſome liquid lava; which laſt, falling back again in a ſpongy ſtate, produces one of thoſe conical hills which we ſee in great number on the vaſt ſides of Mount Ætna, each indicating the diſcharge of a particular eruption. At the ſame time, a jet of flame and ſmoke iſſues from the main crater, proving the internal communication between it and the lava; this diſcharge from the ſummit generally continuing, in a greater or a leſs degree, during the intervals between eruptions. (Fig. 41. repreſents an ideal ſection of Mount Ætna; *a b* is the direct channel, and *b c* is a lateral branch).

Let us now attend to the ſtate of the lava within the mountain, during the courſe of the eruption; and let us ſuppoſe, that a fragment of limeſtone, torn from ſome ſtratum below, has been included in the fluid lava, and carried up with it. By the laws of hydroſtatics, as each portion of this fluid ſuſtains preſſure in proportion to its perpendicular diſtance below the point of diſcharge, that preſſure muſt increaſe with the depth. The ſpecific gravity of ſolid and compact lava is nearly 2.8; and its weight, when in a liquid ſtate, is probably little different. The table ſhews, that the carbonic acid of limeſtone cannot be conſtrained in heat by a preſſure leſs than that of 1708 feet of ſea, which correſponds nearly to 600 feet of liquid lava. As ſoon, then, as our calcareous maſs roſe to within 600 feet of the ſurface, its carbonic acid would quit the lime, and, aſſuming a gaſeous form, would add to the eruptive effervеſcence. And this change would commonly begin in much greater depths, in conſequence of the bubbles of carbonic acid, and other ſubſtances in a gaſeous form, which, riſing with the lava, and through it, would greatly diminiſh the weight of the column, and would render its preſſure on any particular ſpot extremely variable. With all theſe irregularities, however, and interruptions, the preſſure

preſſure would in all caſes, eſpecially where the depth was conſiderable, far ſurpaſs what it would have been under an equal depth of water. Where the depth of the ſtream, below its point of delivery, amounted, then, to 1708 feet, the preſſure, if the heat was not of exceſſive intenſity, would be more than ſufficient to conſtrain the carbonic acid, and our limeſtone would ſuffer no calcination, but would enter into fuſion; and if the eruption ceaſed at that moment, would cryſtallize in cooling along with the lava, and become a nodule of calcareous ſpar. The maſs of lava, containing this nodule, would then conſtitute a real whinſtone, and would belong to the kind called *amygdaloid*. In greater depths ſtill, the preſſure would be proportionally increaſed, till ſulphur, and even water, might be conſtrained; and the carbonate of lime would continue undecompoſed in the higheſt heats.

If, while the lava was in a liquid ſtate, during the eruption or previous to it, a new rent (*d e*, fig. 41.), formed in the ſolid country below the volcano, was met by our ſtream (at *d*), it is obvious that the lava would flow into the aperture with great rapidity, and fill it to the minuteſt extremity, there being no air to impede the progreſs of the liquid. In this manner, a ſtream of lava might be led from below to approach the bottom of the ſea (*f f*), and to come in contact with a bed of looſe ſhells (*g g*), lying on that bottom, but covered with beds of clay, interſtratified, as uſually occurs, with beds of ſand, and other beds of ſhells. The firſt effect of heat would be to drive off the moiſture of the loweſt ſhell-bed, in a ſtate of vapour, which, riſing till it got beyond the reach of the heat, would be condenſed into water, producing a ſlight motion of ebullition, like that of a veſſel of water, when it begins to boil, and when it is ſaid to ſimmer. The beds of clay and ſand might thus undergo ſome heaving and partial derangement, but would ſtill poſſeſs the power of ſtopping, or of very much im-

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peding, the defcent of water from the fea above; fo that the water which had been driven from the fhells at the bottom, would not return to them, or would return but flowly; and they would be expofed dry to the action of heat *.

In this cafe, one of two things would inevitably happen. Either the carbonic acid of the fhells would be driven off by the heat, producing an incondenfable elaftic fluid, which, heaving up or penetrating the fuperincumbent beds, would force its way to the furface of the fea, and produce a fubmarine eruption, as has happened at Santorini and elfewhere; or the volatility of the carbonic acid would be repreffed by the weight of the fuperincumbent water (*k k*), and the fhell-bed, being foftened or fufed by the action of heat, would be converted into a ftratum of limeftone.

The foregoing experiments enable us to decide in any particular cafe, which of thefe two events muft take place, when the heat of the lava and the depth of the fea are known.

The table fhews, that under a fea no deeper than 1708 feet, near one-third of a mile, a limeftone would be formed by proper heat; and that, in a depth of little more than one mile, it would enter into entire fufion. Now, the common foundings of mariners extend to 200 fathoms, or 1200 feet. Lord Mulgrave † found bottom at 4680 feet, or nearly nine-tenths of a mile; and Captain Ellis let down a fea-gage to the depth of 5346 feet ‡. It thus appears, that

* This fituation of things, is fimilar to what happens when fmall-coal is moiftened, in order to make it cake. The duft, drenched with water, is laid upon the fire, and remains long wet, while the heat below fuffers little or no abatement.

† *Voyage towards the North Pole*, p. 142.

‡ *Philofophical Tranfactions*, 1751, p. 212.

that at the bottom of a ſea, which would be ſounded by a line much leſs than double of the uſual length, and leſs than half the depth of that ſounded by Lord Mulgrave, limeſtone might be formed by heat; and that, at the depth reached by Captain Ellis, the entire fuſion would be accompliſhed, if the bed of ſhells were touched by a lava at the extremity of its courſe, when its heat was loweſt. Were the heat of the lava greater, a greater depth of ſea would, of courſe, be requiſite to conſtrain the carbonic acid effectually; and future experiments may determine what depth is required to co-operate with any given temperature. It is enough for our preſent purpoſe to have ſhewn, that the reſult is poſſible in any caſe, and to have circumſcribed the neceſſary force of theſe agents within moderate limits. At the ſame time it muſt be obſerved, that we have been far from ſtretching the known facts; for when we compare the ſmall extent of ſea in which any ſoundings can be found, with that of the vaſt unfathomed ocean, it is obvious, that in aſſuming a depth of one mile or two, we fall very ſhort of the medium. M. de la Place, reaſoning from the phenomena of the tides, ſtates it as highly probable that this medium is not leſs than eleven Engliſh miles *.

If a great part or the whole of the ſuperincumbent maſs conſiſted, not of water, but of ſand or clay, then the depth requiſite to produce theſe effects would be leſſened, in the inverſe ratio of the ſpecific gravity. If the above-mentioned occurrence took place under a maſs compoſed of ſtone firmly bound together by ſome previous operation of nature, the power of the ſuperincumbent maſs, in oppoſing the eſcape of

 carbonic

* "On peut donc regarder au moins comme très probable, que la profondeur "moyenne de la mer n'eſt pas au-deſſous de quatre lieues." De la Place, *Hiſt. de l'Acad. Roy. des Sciences*, année 1776.

carbonic acid, would be very much increaſed by that union and by the ſtiffneſs or tenacity of the ſubſtance. We have ſeen numberleſs examples of this power in the courſe of theſe experiments, in which barrels, both of iron and porcelain, whoſe thickneſs did not exceed one-fourth of an inch, have exerted a force ſuperior to the mere weight of a mile of ſea. Without ſuppoſing that the ſubſtance of a rock could in any caſe act with the ſame advantage as that of a uniform and connected barrel; it ſeems obvious that a ſimilar power muſt, in many caſes, have been exerted to a certain degree.

We know of many calcareous maſſes which, at this moment, are expoſed to a preſſure more than ſufficient to accompliſh their entire fuſion. The mountain of Saleve, near Geneva, is 500 French fathoms, or nearly 3250 Engliſh feet, in height, from its baſe to its ſummit. Its maſs conſiſts of beds, lying nearly horizontal, of limeſtone filled with ſhells. Independently, then, of the tenacity of the maſs, and taking into account its mere weight, the loweſt bed of this mountain, muſt, at this moment, ſuſtain a preſſure of 3250 feet of limeſtone, the ſpecific gravity of which is about 2.65. This preſſure, therefore, is equal to that of 8612 feet of water, being nearly a mile and a half of ſea, which is much more than adequate, as we have ſhewn, to accompliſh the entire fuſion of the carbonate, on the application of proper heat. Now, were an emanation from a volcano, to riſe up under Saleve, and to penetrate upwards to its baſe, and ſtop there; the limeſtone to which the lava approached, would inevitably be ſoftened, without being calcined, and, as the heat retired, would cryſtallize into a ſaline marble.

Some other circumſtances, relating to this ſubject, are very deſerving of notice, and enable us ſtill further to compare the ancient and modern operations of fire.

It

It appears, at firſt ſight, that a lava having once penetrated the ſide of a mountain, all ſubſequent lavas ſhould continue, as water would infallibly do, to flow through the ſame aperture. But there is a material difference in the two caſes. As ſoon as the lava has ceaſed to flow, and the heat has begun to abate, the crevice through which the lava had been paſſing, remains filled with a ſubſtance, which ſoon agglutinates into a maſs, far harder and firmer than the mountain itſelf. This maſs, lying in a crooked bed, and being firmly welded to the ſides of the crevice, muſt oppoſe a moſt powerful reſiſtance to any ſtream tending to purſue the ſame courſe. The injury done to the mountain by the formation of the rent, will thus be much more than repaired; and in a ſubſequent eruption, the lava muſt force its way through another part of the mountain or through ſome part of the adjoining country. The action of heat from below, ſeems in moſt caſes to have kept a channel open through the axis of the mountain, as appears by the ſmoke and flame which is habitually diſcharged at the ſummit during intervals of calm. On many occaſions, however, this ſpiracle ſeems to have been entirely cloſed by the conſolidation of the lava, ſo as to ſuppreſs all emiſſion. This happened to Veſuvius during the middle ages. All appearance of fire had ceaſed for five hundred years, and the crater was covered with a foreſt of ancient oaks, when the volcano opened with freſh vigour in the ſixteenth century.

The eruptive force, capable of overcoming ſuch an obſtacle, muſt be tremendous indeed, and ſeems in ſome caſes to have blown the volcano itſelf almoſt to pieces. It is impoſſible to ſee the Mountain of Somma, which, in the form of a creſcent, embraces Mount Veſuvius, without being convinced that it is a fragment of a large volcano, nearly concentric

with

with the prefent inner cone, which, in fome great eruption, had been deftroyed all but this fragment. In our own times, an event of no fmall magnitude has taken place on the fame fpot; the inner cone of Vefuvius having undergone fo great a change during the eruption in 1794, that it now bears no refemblance to what it was when I faw it in 1785.

The general or partial ftagnation of the internal lavas at the clofe of each eruption feems, then, to render it neceffary, that in every new difcharge, the lava fhould begin by making a violent laceration. And this is probably the caufe of thofe tremendous earthquakes which precede all great eruptions, and which ceafe as foon as the lava has found a vent. It feems but reafonable to afcribe like effects to like caufes, and to believe that the earthquakes which frequently defolate countries not externally volcanic, likewife indicate the protrufion from below of matter in liquid fufion, penetrating the mafs of rock.

The injection of a whinftone-dike into a frail mafs of fhale and fandftone, muft have produced the fame effects upon it that the lava has juft been ftated to produce on the loofe beds of volcanic fcoria. One ftream of liquid whin, having flowed into fuch an affemblage, muft have given it great additional weight and ftrength: fo that a fecond ftream coming like the firft, would be oppofed by a mafs, the laceration of which would produce an earthquake, if it were overcome; or by which, if it refifted, the liquid matter would be compelled to penetrate fome weaker mafs, perhaps at a great diftance from the firft. The internal fire being thus compelled perpetually to change the fcene of its action, its influence might be carried to an indefinite extent: So that the intermittance in point of time, as well as the verfatility in point of place, already remarked as common to the Huttonian and Volcanic fires, are accounted for on our principles.

ples. And it thus appears, that whinſtone poſſeſſes all the properties which we are led by theory to aſcribe to an internal lava.

This connection is curiouſly illuſtrated by an intermediate caſe between the reſults of external and internal fire, diſplayed in an actual ſection of the ancient part of Veſuvius, which occurs in the Mountain of Somma mentioned above. I formerly deſcribed this ſcene in my paper on Whinſtone and Lava; and I muſt beg leave once more to preſs it upon the notice of the public, as affording to future travellers a moſt intereſting field of geological inquiry.

The ſection is ſeen in the bare vertical cliff, ſeveral hundred feet in height, which Somma preſents to the view from the little valley, in form of a creſcent, which lies between Somma and the interior cone of Veſuvius, called the *Atrio del Cavallo.* (Fig. 42. repreſents this ſcene, done from the recollection of what I ſaw in 1785. *a b c* is the interior cone of Veſuvius; *d f g* the mountain of Somma; and *c d e* the *Atrio del Cavallo*). By means of this cliff (*f d* in figure 42. and which is repreſented ſeparately in fig. 44.), we ſee the internal ſtructure of the mountain, compoſed of thick beds (*k k*) of looſe ſcoria, which have fallen in ſhowers; between which thin but firm ſtreams (*m m*) of lava are interpoſed, which have flowed down the outward conical ſides of the mountain. (Fig. 43. is an ideal ſection of Veſuvius and Somma, through the axis of the cones, ſhewing the manner in which the beds of ſcoria and of lava lie upon each other; the extremities of which beds are ſeen edgewiſe in the cliff at *m m* and *k k*, fig. 42, 43, and 44.).

This aſſemblage of ſcoria and lava is traverſed abruptly and vertically, by ſtreams of ſolid lava (*n n*, fig. 44.), reaching from top to bottom of the cliff. Theſe laſt I conceive to have flowed in rents of the ancient mountain, which rents had acted

as

as pipes through which the lavas of the lateral eruptions were conveyed to the open air. This ſcene preſents to the view of an attentive obſerver, a real ſpecimen of thoſe internal ſtreams which we have juſt been conſidering in ſpeculation, and they may exhibit circumſtances deciſive of the opinions here advanced. For, if one of theſe ſtreams had formerly been connected with a lateral eruption, diſcharged at more than 600 feet above the *Atrio del Cavallo*, it might poſſibly contain the carbonate of lime. But could we ſuppoſe that depth to extend to 1708 feet, the interference of air-bubbles, and the action of a ſtronger heat than was merely required for the fuſion of the carbonate, might have been overcome.

Perhaps the height of Veſuvius has never been great enough for this purpoſe. But could we ſuppoſe Ætna to be cleft in two, and its ſtructure diſplayed, as that of Veſuvius has juſt been deſcribed, there can be no doubt that internal ſtreams of lava would be laid open, in which the preſſure muſt have far exceeded the force required to conſtrain the carbonic acid of limeſtone; ſince that mountain occaſionally delivers lavas from its ſummit, placed 10.954 feet above the level of the Mediterranean *, which waſhes its baſe. I recollect having ſeen, in ſome parts of Ætna, vaſt chaſms and crags, formed by volcanic revolutions, in which vertical ſtreams of lava, ſimilar to thoſe of Somma, were apparent. But my attention not having been turned to that object till many years afterwards, I have only now to recommend the inveſtigation of this intereſting point to future travellers.

What has been ſaid of the heat conveyed by internal volcanic ſtreams, applies equally to that deeper and more general heat by which the lavas themſelves are melted and propelled upwards.

* *Phil. Tranſ.* 1777, p. 595.

upwards. That they have been really ſo propelled, from a great internal maſs of matter, in liquid fuſion, ſeems to admit of no doubt, to whatever cauſe we aſcribe the heat of volcanoes. It is no leſs obvious, that the temperature of that liquid muſt be of far greater intenſity than the lavas, flowing from it, can retain when they reach the ſurface. Independently of any actual eruption, the body of heat contained in this vaſt maſs of liquid, muſt diffuſe itſelf through the ſurrounding ſubſtances, the intenſity of the heat being diminiſhed by ſlow gradations, in proportion to the diſtance to which it penetrates. When, by means of this progreſſive diffuſion, the heat has reached an aſſemblage of looſe marine depoſites, ſubject to the preſſure of a great ſuperincumbent weight, the whole muſt be agglutinated into a maſs, the ſolidity of which will vary with the chemical compoſition of the ſubſtance, and with the degree of heat to which each particular ſpot has thus been expoſed. At the ſame time, analogy leads us to ſuppoſe, that this deep and extenſive heat muſt be ſubject to viciſſitudes and intermiſſions, like the external phenomena of volcanoes. We have endeavoured to explain ſome of theſe irregularities, and a ſimilar reaſoning may be extended to the preſent caſe. Having ſhewn, that ſmall internal ſtreams of lava tend ſucceſſively to pervade every weak part of a volcanic mountain, we are led to conceive, that the great maſſes of heated matter juſt mentioned, will be ſucceſſively directed to different parts of the earth; ſo that every looſe aſſemblage of matter, lying in a ſubmarine and ſubterranean ſituation, will, in its turn, be affected by the indurating cauſe; and the influence of internal volcanic heat will thus be circumſcribed within no limits but thoſe of the globe itſelf.

A SERIES of undoubted facts prove, that all our ſtrata once lay in a ſituation ſimilar in all reſpects to that in which the marine depoſites juſt mentioned have been ſuppoſed to lie.

THE inhabitant of an unbroken plain, or of a country formed of horizontal ſtrata, whoſe obſervations have been confined to his native ſpot, can form no idea of thoſe truths, which at every ſtep in an alpine diſtrict force themſelves on the mind of a geological obſerver. Unfortunately for the progreſs of geology, both London and Paris, are placed in countries of little intereſt; and thoſe ſcenes by which the principles of this ſcience are brought into view in the moſt ſtriking manner, are unknown to many perſons beſt capable of appreciating their value. The moſt important, and at the ſame time, the moſt aſtoniſhing truth which we learn by any geological obſervations, is, that rocks and mountains now placed at an elevation of more than two miles above the level of the ſea, muſt at one period have lain at its bottom. This is undoubtedly true of thoſe ſtrata of limeſtone which contain ſhells; and the ſame concluſion muſt be extended to the circumjacent ſtrata. The imagination ſtruggles againſt the admiſſion of ſo violent a poſition; but muſt yield to the force of unqueſtionable evidence; and it is proved by the example of the moſt eminent and cautious obſervers, that the concluſion is inevitable *.

Another queſtion here occurs, which has been well treated by Mr Playfair. Has the ſea retreated from the mountains? or have they riſen out of the ſea? He has ſhewn, that the balance of probability is incomparably in favour of the latter ſuppoſition; ſince, in order to maintain the former, we muſt diſpoſe of an enormous maſs of ſea, whoſe depth is ſeveral miles, and whoſe baſe is greater than the ſurface of the whole ſea. Whereas the elevation of a continent out of a ſea like ours, would not change its level above a few feet; and even were a great derangement thus occa-

ſioned,

* Saussure, *Voyages dans les Alpes*, tom. ii. p. 99.—104.

fioned, the water would eafily find its level without the af-fiftance of any extraordinary fuppofition. The elevation of the land, too, is evinced by what has occafionally happened in volcanic regions, and affords a complete folution of the contortion and erection of ftrata, which are almoft univerfally admitted to have once lain in a plane and horizontal pofition.

Whatever opinion be adopted as to the mode in which the land and the water have been feparated, no one doubts of the ancient fubmarine fituation of the ftrata.

An important feries of facts proves, that they were likewife fubterranean. Every thing indicates that a great quantity of matter has been removed from what now conftitutes the furface of our globe, and enormous depofites of loofe fragments, evidently detached from maffes fimilar to our common rock, evince the action of fome very powerful agent of deftruction. Analogy too, leads us to believe, that all the primary rocks have once been covered with fecondary; yet, in vaft diftricts, no fecondary rock appears. In fhort, geologifts feem to agree in admitting the general pofition, that very great changes of this kind have taken place in the folid furface of the globe, however much they may differ as to their amount, and as to their caufes.

Dr Hutton afcribed thefe changes to the action, during very long time, of thofe agents, which at this day continue flowly to corrode the furface of the earth; frofts, rains, the ordinary floods of rivers, &c. which he conceives to have acted always with the fame force, and no more. But to this opinion I could never fubfcribe, having early adopted that of Saussure, in which he is joined by many of the continental geologifts. My conviction was founded upon the infpection of thofe facts in the neighbourhood of Geneva, which he has adduced in fupport of his opinion. I was then convinced, and I ſtill believe, that vaſt torrents, of depth ſufficient to overtop our mountains, have ſwept along the ſurface of the earth, excavating vallies, undermining mountains, and carrying away whatever was unable to reſiſt ſuch powerful corroſion. If ſuch agents have been at work in the Alps, it is difficult to conceive that our countries ſhould have been ſpared. I made it therefore my buſineſs to ſearch for traces of ſimilar operations here. I was not long in diſcovering ſuch in great abundance; and, with the help of ſeveral of my friends, I have traced the indications of vaſt torrents in this neighbourhood, as obvious as thoſe I formerly ſaw on Saleve and Jura. Since I announced my opinion on this ſubject, in a note ſubjoined to my paper on Whinſtone and Lava, publiſhed in the fifth volume of the *Tranſactions* of this Society, I have met with many confirmations of theſe views. The moſt important of theſe are derived from the teſtimony of my friend Lord SELKIRK, who has lately met with a ſeries of ſimilar facts in North America.

IT would be difficult to compute the effects of ſuch an agent; but if, by means of it, or of any other cauſe, the whole maſs of ſecondary ſtrata, in great tracts of country, has been removed from above the primary, the weight of that maſs alone muſt have been ſufficient to fulfil all the conditions of the Huttonian Theory, without having recourſe to the preſſure of the ſea. But when the two preſſures were combined, how great muſt have been their united ſtrength!

WE are authoriſed to ſuppoſe, that the materials of our ſtrata, in this ſituation, underwent the action of fire. For volcanoes have burnt long before the earlieſt times recorded in hiſtory, as appears by the magnitude of ſome volcanic mountains; and it can ſcarcely be doubted, that their fire has acted without any material ceſſation ever ſince the ſurface of our globe acquired its preſent prefent form. In extending that fame influence to periods of ftill higher antiquity, when our ftrata lay at the bottom of the fea, we do no more than afcribe permanence to the exifting laws of nature.

The combination of heat and compreffion refulting from thefe circumftances, carries us to the full extent of the Huttonian Theory, and enables us, upon its principles, to account for the igneous formation of all rocks from loofe marine depofites.

The fand would thus be changed to fandftone; the fhells to limeftone; and the animal and vegetable fubftances to coal.

Other beds, confifting of a mixture of various fubftances, would be ftill more affected by the fame heat. Such as contained iron, carbonate of lime, and alkali, together with a mixture of various earths, would enter into thin fufion, and, penetrating through every crevice that occurred, would, in fome cafes, reach what was then the furface of the earth, and conftitute lava: in other cafes, it would congeal in the internal rents, and conftitute porphyry, bafalt, greenftone, or any other of that numerous clafs of fubftances, which we comprehend under the name of *whinftone*. At the fame time, beds of fimilar quality, but of compofition fomewhat lefs fufible, would enter into a ftate of vifcidity, fuch as many bodies pafs through in their progrefs towards fufion. In this ftate, the particles, though far from poffeffing the fame freedom as in a liquid, are fufceptible of cryftalline arrangement *; and the fubftance

* This ftate of vifcidity, with its numberlefs modifications, is deferving of great attention, fince it affords a folution of fome of the moft important geological queftions. The mechanical power exerted by fome fubftances, in the act of affuming a cryftalline form, is well known. I have feen a fet of large and broad cryftals

ſubſtance, which, in this ſluggiſh ſtate, would be little diſpoſed to move, being confined in its original ſituation by contiguous beds of more refractory matter, would cryſtallize, without undergoing any change of place, and conſtitute one of thoſe beds of whinſtone, which frequently occur interſtratified with ſandſtone and limeſtone.

In other caſes where the heat was more intenſe, the beds of ſand, approaching more nearly to a ſtate of fuſion, would acquire ſuch tenacity and toughneſs, as to allow themſelves to be bent and contorted, without laceration or fracture, by the influence of local motions, and might aſſume the ſhape and character of primary ſchiſtus: the limeſtone would be highly cryſtallized, and would become marble, or, entering into thin fuſion, would penetrate the minuteſt rents in the form of calcareous ſpar. Laſtly, when the heat was higher ſtill, the ſand itſelf would be entirely melted, and might be converted, by the ſubſequent effects of ſlow cooling, into granite, ſienite, &c.; in ſome caſes, retaining traces of its original ſtratification, and conſtituting gneiſs and ſtratified granite; in others, flowing into the crevices, and forming veins of perfect granite.

In conſequence of the action of heat, upon ſo great a quantity of matter, thus brought into a fluid or ſemifluid ſtate, and in which, notwithſtanding the great preſſure, ſome ſubſtances would be volatilized, a powerful heaving of the ſuperincumbent maſs muſt have taken place; which, by repeated efforts, ſucceeding

cryſtals of ice, like the blade of a knife, formed in a maſs of clay, of ſuch ſtiffneſs, that it had juſt been uſed to make cups for chemical purpoſes. In many of my former experiments, I found that a fragment of glaſs made from whinſtone or lava, when placed in a muffle heated to the melting point of ſilver, aſſumed a cryſtalline arrangement, and underwent a complete change of character. During this change, it became ſoft, ſo as to yield to the touch of an iron rod; yet retained ſuch ſtiffneſs, that, lying untouched in the muffle, it preſerved its ſhape entirely; the ſharp angles of its fracture not being in the leaſt blunted.

ceeding each other from below, would at laſt elevate the ſtrata into their preſent ſituation.

The Huttonian Theory embraces ſo wide a field, and comprehends the laws of ſo many powerful agents, exerting their influence in circumſtances and in combinations hitherto untried, that many of its branches muſt ſtill remain in an unfiniſhed ſtate, and may long be expoſed to partial and plauſible objections, after we are ſatisfied with regard to its fundamental doctrines. In the mean time I truſt, that the object of our purſuit has been accompliſhed, in a ſatisfactory manner, by the fuſion of limeſtone under preſſure. This ſingle reſult affords, I conceive, a ſtrong preſumption in favour of the ſolution which Dr Hutton has advanced of all the geological phenomena; for, the truth of the moſt doubtful principle which he has aſſumed, has thus been eſtabliſhed by direct experiment.

APPEN-

APPENDIX.

No. I.

SPECIFIC GRAVITY OF SOME OF THE FOREGOING RESULTS.

AS many of the artificial limeſtones and marbles produced in theſe experiments, were poſſeſſed of great hardneſs and compactneſs, and as they had viſibly undergone a great diminution of bulk, and felt heavy in the hand, it ſeemed to me an object of ſome conſequence to aſcertain their ſpecific gravity, compared with each other, and with the original ſubſtances from which they were formed. As the original was commonly a maſs of chalk in the lump, which, on being plunged into water, begins to abſorb it rapidly, and continues to do ſo during a long time, ſo as to vary the weight at every inſtant, it was impoſſible, till the abſorption was complete, to obtain any certain reſult; and to allow for the weight thus gained, required the application of a method different from that uſually employed in eſtimating ſpecific gravity.

In the common method, the ſubſtance is firſt weighed in air, and then in water; the difference indicating the weight of water diſplaced, and being conſidered as that of a quantity of water equal in bulk to the ſolid body. But as chalk, when ſaturated with water, is heavier, by about one-fourth, than when dry, it is evident, that its apparent weight, in water, muſt be increaſed, and the apparent loſs of weight diminiſhed exactly to that amount. To have a juſt eſtimate, then, of the quantity of water diſplaced by the ſolid body, the apparent loſs of weight muſt be increaſed, by adding the abſorption to it.

Two diſtinct methods of taking ſpecific gravity thus preſent themſelves, which it is of importance to keep ſeparate, as each of them is applicable to a particular claſs of ſubjects.

One of theſe methods, conſiſts in comparing a cubic inch of a ſubſtance in its dry ſtate, allowing its pores to have their ſhare in conſtituting its bulk, with a cubic inch of water.

The other depends upon comparing a cubic inch of the ſolid matter of which the ſubſtance is compoſed, independently of vacuities, and ſuppoſing the whole reduced to perfect ſolidity, with a cubic inch of water.

Thus, were an architect to compute the efficacy of a given bulk of earth, intended to load an abutment, which earth was dry, and ſhould always remain ſo, he would undoubtedly follow the firſt of theſe modes: Whereas, were a farmer to compare the ſpecific gravity of the ſame earth with that of any other ſoil, in an agricultural point of view, he would uſe the ſecond mode, which is involved in that laid down by Mr Davy.

As our object is to compare the ſpecific denſity of theſe reſults, and to aſcertain to what amount the particles have approached

proached each other, it ſeems quite evident that the firſt mode is ſuited to our purpoſe. This will appear moſt diſtinctly, by inſpection of the following Table, which has been conſtructed ſo as to include both.

TABLE OF SPECIFIC GRAVITIES.

I.	II. Weight in air, dry.	III. Weight in water.	IV. Weight in air, wet.	V. Difference between Columns II. & III.	VI. Difference between Columns II. & IV. or abforption.	VII. Abforption *per cent.*	VIII. Sum of Columns V. and VI.	IX. Specific gravity by common mode.	X. Specific gravity by new mode.
1.	125.90	77.55	135.65	47.35	9.75	7.74	57.10	2.604	2.204
2.	9.94	6.13	9.99	3.81	0.05	0.50	3.86	2.609	2.575
3.	15.98	9.70	16.02	6.28	0.04	0.25	6.32	2.544	2.528
4.	5.47	3.33	5.48	2.14	0.01	0.18	2.15	2.556	2.544
5.	18.04	10.14	18.06	7.90	0.02	0.11	7.92	2.283	2.277
6.	6.48	3.74	7.10	2.74	0.62	9.56	3.36	2.365	1.928
7.	10.32	5.97	10.36	4.35	0.04	0.39	4.39	2.372	2.350
8.	54.57	31.30	55.23	23.27	0.66	1.21	23.93	2.345	2.280
9.	72.27	41.10	76.13	31.17	3.86	5.34	35.03	2.318	2.063
10.	37.75	21.15	38.30	16.60	0.55	1.45	17.15	2.274	2.201
11.	21.21	12.55	21.26	8.66	0.05	0.24	8.71	2.449	2.435
12. Marble.	18.59	11.56	18.61	7.03	0.02	0.18	7.05	2.644	2.636
13. Chalk.	504.15	302.40	623.20	201.75	119.05	23.61	320.80	2.498	1.571
14. Average Chalk.	444.30	264.35	550.80	179.95	106.50	23.97	286.45	2.469	1.551
15. rammed Powder.	283.97	—	—	—	—	—	198.65	—	1.429

EXPLANATION.

Column I. contains the number affixed to each of the fpecimens, whofe properties are expreffed in the table.

The

The first eleven are the same with those used in the paper read in this Society on the 30th of August 1804, and published in Nicholson's *Journal* for October following, and which refer to the same specimens. No. 12. Is a specimen of yellow marble, bearing a strong resemblance to No. 3. No. 13. A specimen of chalk. No. 14. Shews the average of three trials with chalk. No. 15. Some pounded chalk, rammed in the manner followed in these experiments. In order to ascertain its specific gravity, I rammed the powder into a glass-tube, previously weighed; then, after weighing the whole, I removed the chalk, and filled the same tube with water. I thus ascertained, in a direct manner, the weight of the substance, as stated in Column II., and that of an equal bulk of water, stated in Column VIII.

Column II. Weight of the substance, dry in air, after exposure, during several hours to a heat of 212° of Fahrenheit.

Column III. Its weight in water, after lying long in the liquid, so as to perform its full absorption; and all air-bubbles being carefully removed.

Column IV. Weight in air, wet. The loose external moisture being removed by the touch of a dry cloth; but no time being allowed for evaporation.

Column V. Difference between Columns II. and III., or apparent weight of water displaced.

Column VI. Difference between Columns II. and IV., or the absorp tion

Column VII. Absorption reduced to a *per centage* of the dry substance.

Column VIII. Sum of Columns V. and VI., or the real weight of water displaced by the body.

Column IX. Specific gravity, by the common mode, resulting from the division of Column II. by Column V.

Column X. Specific gravity, in the new mode, resulting from the division of Column II. by Column VIII.

The

The ſpecific gravities aſcertained by the new mode, and expreſſed in Column X. correſpond very well to the idea which is formed of their comparative denſities, from other circumſtances, their hardneſs, compact appearance, ſuſceptibility of poliſh, and weight in the hand.

The caſe is widely different, when we attend to the reſults of the common method contained in Column IX. Here the ſpecific gravity of chalk is rated at 2.498, which exceeds conſiderably that of a majority of the reſults tried. Thus, it would appear, by this method, that chalk has become lighter by the experiment, in defiance of our ſenſes, which evince an increaſe of denſity.

This ſingular reſult ariſes, I conceive, from this, that, in our ſpecimens, the faculty of abſorption has been much more decreaſed than the poroſity. Thus, if a piece of crude chalk, whoſe ſpecific gravity had previouſly been aſcertained by the common mode, and then well dried in a heat of 212°, were dipped in varniſh, which would penetrate a little way into its ſurface; and, the varniſh having hardened, the chalk were weighed in water, it is evident, that the apparent loſs of weight would now be greater by 23.61 *per cent.* of the dry weight, than it had been when the unvarniſhed chalk was weighed in water; becauſe the varniſh, cloſing the ſuperficial pores, would quite prevent the abſorption, while it added but little to the weight of the maſs, and made no change on the bulk. In computing, then, the ſpecific gravity, by means of this laſt reſult, the chalk would appear very much lighter than at firſt, though its denſity had, in fact, been increaſed by means of the varniſh.

A similar effect ſeems to have been produced in ſome of theſe reſults, by the agglutination or partial fuſion of part of the ſubſtance, by which ſome of the pores have been ſhut out from the water.

This

THIS view derives ſome confirmation from an inſpection of Columns VI. and VII.; the firſt of which expreſſes the abſorption; and the ſecond, that reſult, reduced to a *per centage* of the original weight. It there appears, that whereas chalk abſorbs 23.97 *per cent.*, ſome of our reſults abſorb only 0.5, or ſo low as 0.11 *per cent.* So that the power of abſorption has been reduced from about one-fourth, to leſs than the five hundredth of the weight.

I HAVE meaſured the diminution of bulk in many caſes, particularly in that of No. 11. The chalk, when crude, ran to the 75th degree of WEDGWOOD's gage, and ſhrunk ſo much during the experiment, that it ran to the 161ſt.; the difference amounting to 86 degrees. Now, I find, that WEDGWOOD's gage tapers in breadth, from 0.5 at zero of the ſcale, to 0.3 at the 240th degree. Hence, we have for one degree 0.000833. Conſequently, the width, at the 75th degree, amounts to 0.437525; and at the 161ſt, to 0.365887. Theſe numbers, denoting the linear meaſure of the crude chalk, and of its reſult under heat and compreſſion, are as 100 to 83.8; or, in ſolid bulk, as 100 to 57.5. Computing the denſities from this ſource, they are as 1 to 1.73. The ſpecific gravities in the Table, of the chalk, and of this reſult, are as 1.551 : 2.435; that is, as 1 to 1.57. Theſe concluſions do not correſpond very exactly; but the chalk employed in this experiment, was not one of thoſe employed in determining average ſpecific gravity in the Table; and other circumſtances may have contributed to produce irregularity. Comparing this chalk with reſult ſecond, we have 1.551 : 2.575 ſo 1 : 1.6602.

TABLE

No. II.

TABLE,

CONTAINING THE REDUCTION OF THE FORCES MENTIONED IN CHAP. VII. TO A COMMON STANDARD.

I. Number of experiment referred to in Chap. VII.	II. Bore, in decimals of an inch.	III. Preſſure in hundred weights.	IV. Temperature by WEDGWOOD's pyrometer.	V. Depth of ſea in feet.	VI Ditto in miles.	VII. Preſſure, expreſſed in atmoſpheres
1	0.75	3	22	1708 05	0.3235	51.87
2	0.75	3	25	1708.05	0.3235	51.87
3	0.75	10	20	5693.52	1.0783	172.92
4	0.75	10	31	5693 52	1.0783	172.92
5	0.75	10	41	5693.52	1.0783	172.92
6	0.75	10	51	5693.52	1.0783	172.92
7	0.75	10	—	5693.52	1.0783	172.92
8	0.54	2	—	2196.57	0.4160	66.71
9	0.54	{ 4	—	4393.14	0.8320	133.43
		{ 8.1	—	8896.12	1.6848	270.19
10	0.75	3	21	1708.05	0.3235	51.87
11	0.75	4	25	2277.41	0.4313	69.70
12	0.75	5	—	2846.76	0.5396	86.46

EXPLANATION.

EXPLANATION.

COLUMN I. contains the number of the experiment, as referred to in the text. Column II. The bore of the barrel uſed, in decimals of an inch. Column III. The abſolute force applied to the barrel, in hundred-weights. Column IV. The temperature, in WEDGWOOD's ſcale. Column V. The depth of ſea at which a force of compreſſion would be exerted equal to that ſuſtained by the carbonate in each experiment, expreſſed in feet. Column VI. The ſame in miles. Column VII. Compreſſing force, expreſſed in atmoſpheres.

BOTH Tables were computed ſeparately, by a friend, Mr J. JARDINE, and myſelf.

THE following data were employed.

AREA of a circle of which the diameter is unity, 0.785398.

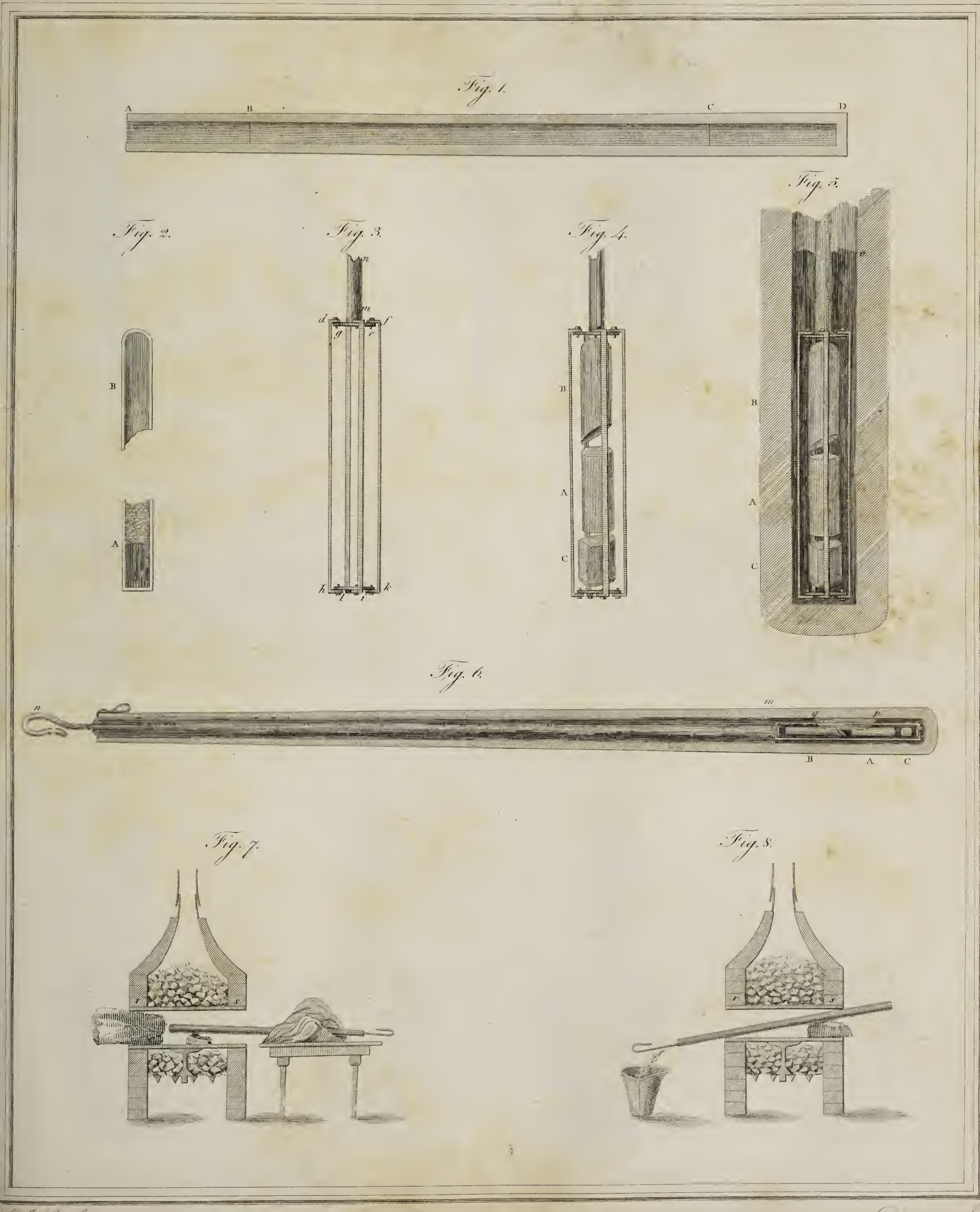
WEIGHT of a cubic foot of diſtilled water, according to Profeſſor ROBISON, 998.74 ounces avoirdupois.

MEAN ſpecific gravity of ſea-water, according to BLADH, 1.0272.

MEAN heighth of the barometer at the level of the ſea 29.91196 Engliſh inches, according to LAPLACE.

SPECIFIC gravity of mercury, according to CAVENDISH and BRISSON, 13.568.

FINIS



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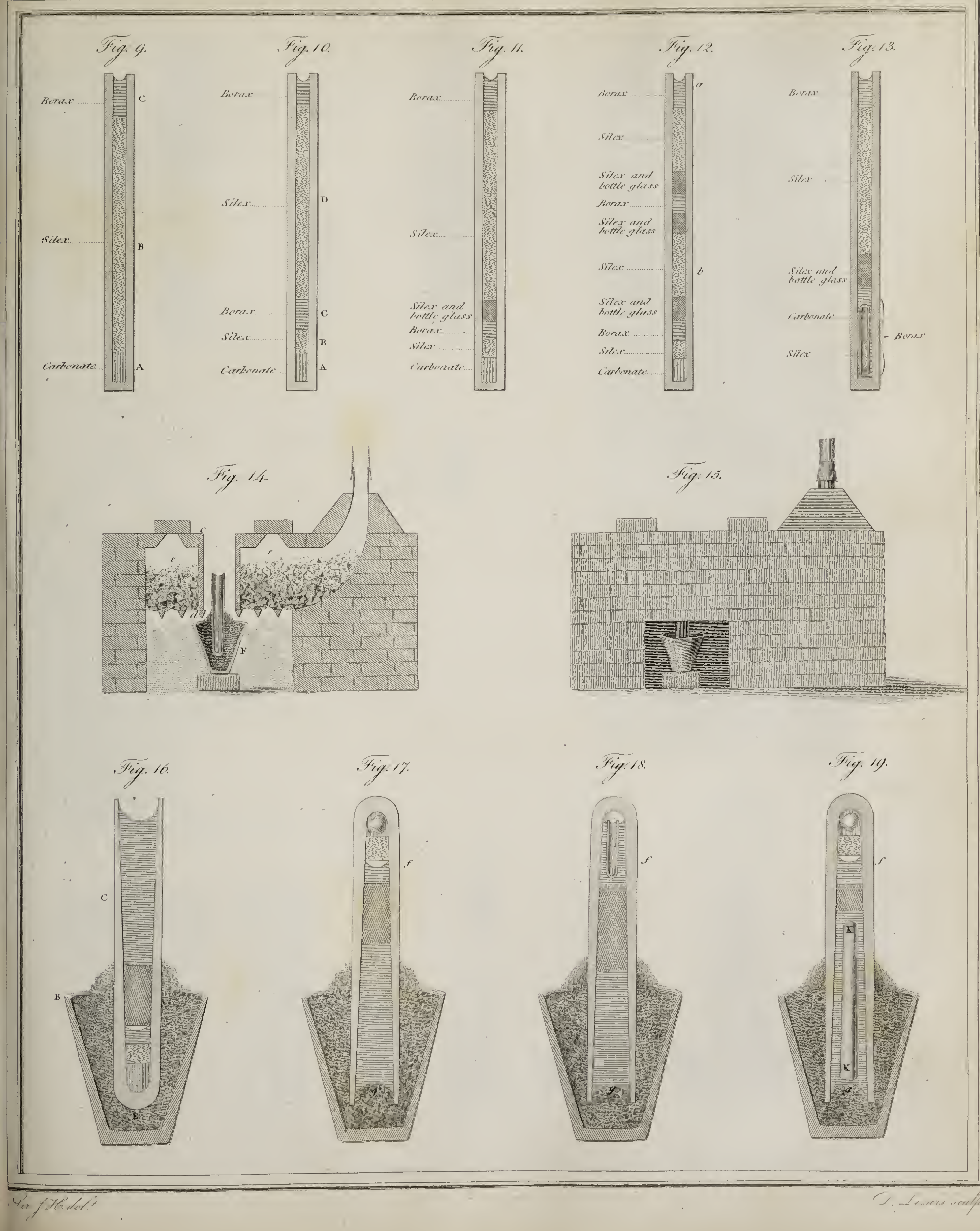
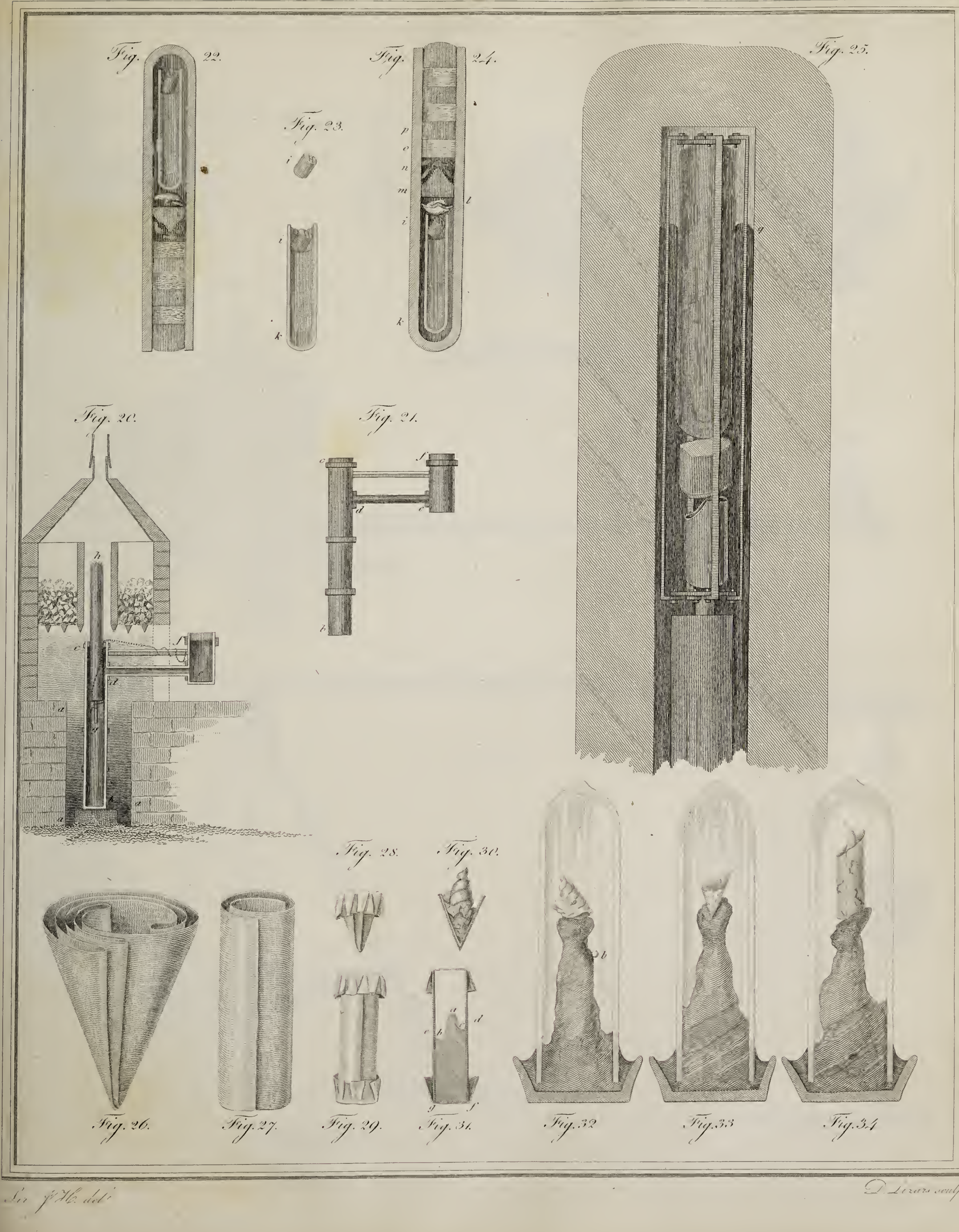
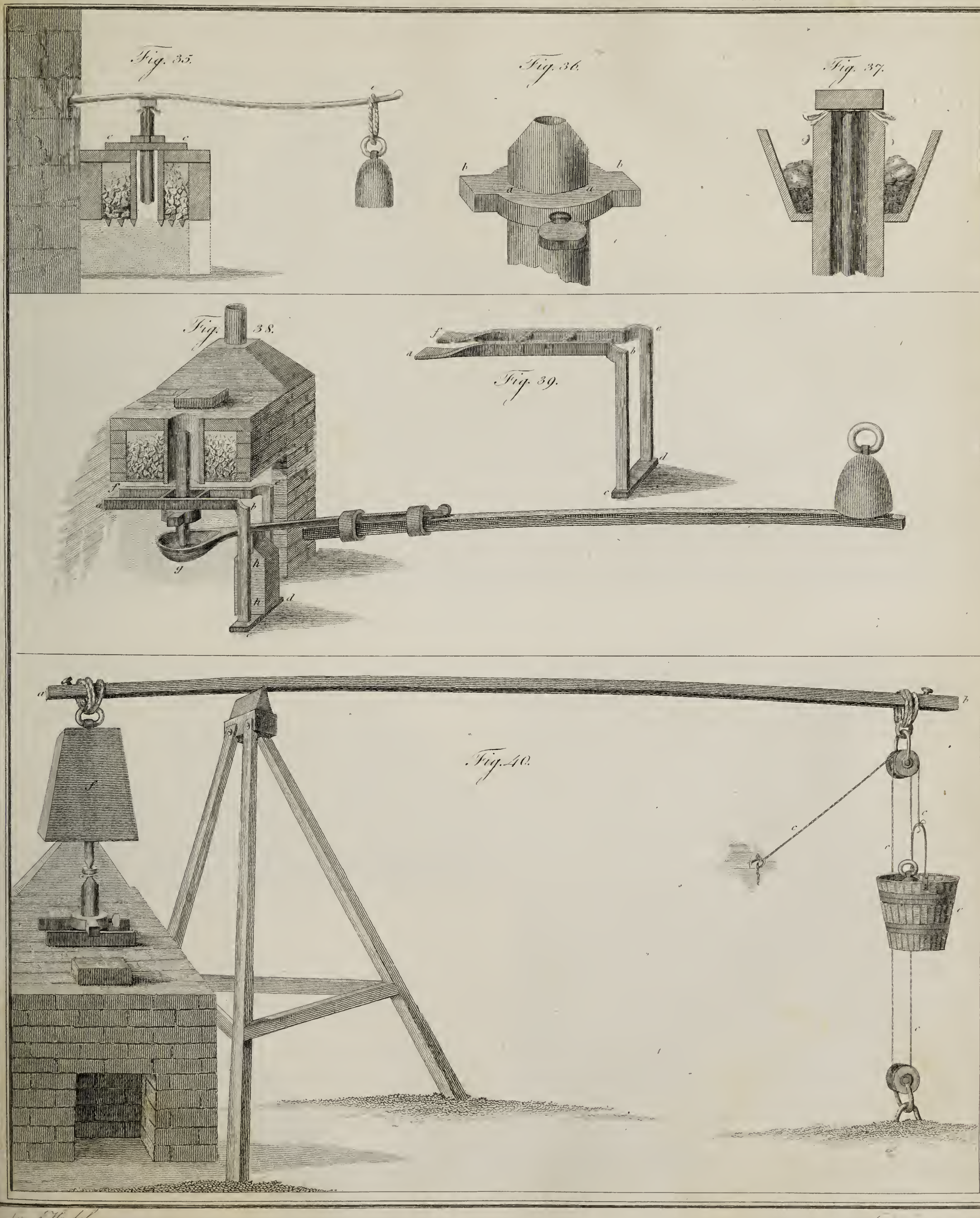


Sir J.H. del.

D. Lizars sculp.



Sir J. H. del.   D. Lizars sculp.



Sir J.H. del. D. Lizars sculp

PLATE V.